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ABSTRACT

This report is an evaluation, using randomized experimental/control design, of a program involving negotiated contractual agreements (MAP) between prisoners and parole boards for specific parole dates contingent on performance in work, training and treatment activities. Contracts were generally for less than six months. Both feasibility and effectiveness of the model were analyzed. Reasonably high levels of acceptance were found among both prisoners and correctional officials in Wisconsin and Arizona; but in California, where the model involved direct release to a community correctional center, administrative obstacles hindered full implementation. Contract cancellations were almost always the result of disciplinary infractions rather than failure to satisfy work or training requirements; prisoner-initiated withdrawals were rare. At release, prisoners judged that MAP had provided them greatest service through more certainty of release, facilitating planning outside prison, and the opportunity for earlier release, but that MAP had made less substantial differences in improving staff interest, access to prison programs, or operation of those programs. (Author/RC)

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Resource Document #5

PAROLE
CORRECTIONS
PROJECT

MAP MARKERS

RESEARCH
AND
EVALUATION
OF THE
MUTUAL
AGREEMENT
PROGRAM

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ABSTRACT

This report is an evaluation, using randomized experimental/control design, a program involving negotiated contractual agreements (MAP) between prisoners and parole boards for specific parole dates contingent on performance in work, training and treatment activities. Contracts were generally for less than six months. Both feasibility and effectiveness of the model were analyzed. Reasonably high levels of acceptance were found among both prisoners and correctional officials in Wisconsin and Arizona; but in California, where the model involved direct release to a community correctional center, administrative obstacles hindered full implementation. Contract cancellations were almost always the result of disciplinary infractions rather than failure to satisfy work or training requirements; prisoner-initiated withdrawals were rare. At release, prisoners judged that MAP had provided them greatest service through more certainty of release, facilitating planning outside prison, and the opportunity for earlier release, but that MAP had made less substantial difference in improving staff interest, access to prison programs, or operation of those programs. Analyses yielded no statistically significant differences favoring experimental subjects on time served in prison, success in acquiring or holding employment, or recidivism within six months after release. For future applications, stronger measures are advised (including arbitration and collective representation) to lessen the risks of coercive utilization.

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FOREWORD

by Fred Cohen*

Like too many of my colleagues trained in law, encounters with research methods and statistics have for me all the appeal of the bubonic plague. I recognize also that my distant professional relatives who work at research, to say nothing of those who work at corrections, are not always convinced of the value of a legal approach to an area traditionally regarded as their exclusive domain. In abandoning my fear of infection, if only for the moment, I ask that the "no trespassing" sign also be abandoned, if only for the moment.

My interest in correctional arrangements, particularly from the viewpoint of the law, is a longstanding one. It was only yesterday when it was accurate to say that the law of corrections is relatively easy to grasp - there simply is none. The conversion from accused to convicted resulted not only in a dramatic shift of status but in an almost total shift of power to the State over the life and liberty of the individual.

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Today, largely due to judicial intervention and against massive opposition from the corrections establishment, corrections is no longer a legal no-man's land. True, the changes have been more formal than real; power remains largely unaffected since most of the judicially imposed changes relate to procedure as opposed to substantive rights; and, true also that we may now be returning to an era of judicial restraint under the rhuberic of the "hands off" doctrine. But the point remains that persons convicted of crime can no longer be viewed as non-persons, as objects to be acted upon for any purpose and by any procedure deemed acceptable by the keepers of the system.

In the area of correctional litigation, one may lose sight of the true significance of the process if only wins and losses are counted. Judges have been educated about processes and institutions, more and more lawyers have been attracted to the area, and persons under correctional restraint have had their expectations lifted, although regrettably at times beyond what can realistically be delivered.

I can't help but believe that even a modest effort to implement contract parole, or Mutual Agreement Programming (MAP), would not have been possible without the repeated judicial excursions, if not actual incursions, into the prison and parole processes. When lawyers began to ask about program objectives and results, to question the ability of parole boards to predict future behavior, to question the validity and reliability of information used for decisions vital to liberty; the arbitrary and capricious nature of correctional decision making was revealed. MAP, it seems to me, is an effort to respond to some of these revelations through what appears to be a device to share decision making, reduce the corrosive effects of the uncertainty of a release date, encourage the utilization of existing resources and stimulate the availability of others, and, ultimately, provide acceptable alternatives to future criminality.

At one level, this evaluation of MAP in Arizona and Wisconsin, with passing reference to the abortive effort to implement and evaluate a similar program in California, is no cause for celebration. There is simply no dramatic evidence of positive program effects in the area of post-release employment adjustment or in recidivism when experimentals are compared with controls. Six months after release, the arrest-free rates were found to be identical for experimentals and controls. When the direction, rather than the magnitude, of post-release differences on such items as job retention, full-time employment, and earning level, was explored experimentals consistently fared worse than those who did not go through MAP.

Jim Robison, Research Director, in his conclusion, correctly it seems to me, argues that to view the disappointing results and conclude that MAP is worthless would be a mistake. He points out that when this demonstration program began, there were two fundamental unanswered questions, not just one. There was not only the question of whether MAP would lead to enhanced performance, but whether the rigors and implications of a signed and binding contract could be tolerated in a system accustomed to maximal discretion and unilateral control. The latter question, he believes, has been answered and in the affirmative.

The results of this demonstration program will not cause correctional officials to rush in as though MAP equates with the discovery of gold. On the other hand, I remain sufficiently cynical to believe that if funding sources provide the gold and some decent P.R. possibilities exist, MAP has a good chance for wide acceptance, albeit limited use; use confined to those prisoners who would be the best bets anyway. Thus, I suppose that it will be the Department of Labor or L.E.A.A., and not A.C.A. or some parole board, that ultimately decides whether MAP is sound.

I cannot imagine lawyers relaxing the pressures of litigation because of MAP; and this even if the Robison Report found extraordinary levels of accomplishment. The basic challenge, after all, to the discretionary release aspects of parole is that time served is not related to the seriousness of the underlying offense. The moment that psychological profiles, prison performance, or predictions of future criminality enter the picture we enter the subjective land of "who" and leave the objective grounds of "what."

At another level, can we safely leave to administrators the unchallenged judgment of who is eligible, what conditions are acceptable, and procedures and criteria for termination? Does the appearance of a signed agreement in fact denote voluntariness or is it merely coercion in another form? If MAP is truly a process of agreement--another term for contracting--doesn't the inmate need advice and counselling, if not actual representation, in the bargaining process? I would venture to say that the same administrator who would not dream of selling his house or contracting for the sale of goods without legal advice would find it unthinkable to provide inmates who are bargaining for their liberty with such advice.

The existence of problems and the raising of questions ought not to be taken as an unthinking broadside at MAP. Indeed, the conceptual seeds for some reform may be here. The very notion of a prisoner, not long ago described as a slave of the State, sitting down to negotiate a type of performance contract can be viewed as having considerable ameliorative potential. Making such a program truly voluntary would enhance the appeal. If certainty on time served is not to be achieved at the time of judicial sentencing by adoption of the just desserts, or proportionality model in sentencing, then post-sentencing certainty may be the best we can get.

ACKNOWLEDGEMENTS

My deep gratitude:

First, to the hundreds of prisoners in Arizona and Wisconsin, and to the few in California, who tolerantly completed fifteen pages of questionnaire items before release, and then graciously continued to supply information after their return to the community.

Second, to the three state project coordinators--Donald MacDonald in California, Gerald Mills in Wisconsin, and Victor Reyes in Arizona, for diligently handling burdensome data collection tasks despite the more pressing obligation they faced in managing program operations.

Third, to the data coordinator, Margo Robison, for instrument and data systems development, monitoring of information flow, management of codification and data conversion, and consultation on analysis.

PREFACE

It would be nice if a prisoner could know when he'd be going home. That single sentence carries most of the explanation for my association, as research director, with the Parole-Corrections-Project experiment in Mutual Agreement Programming.

Departments of corrections and parole boards have sometimes been referred to as lawless agencies. A major intent of models based on formalization of contractual arrangements between captives and captors is to establish greater accountability among those responsible for serving society through the management of its prisoners. To a large extent however, there remains a heavy dependance on good faith between the bargaining parties, since the level of empowerment for offenders and the means of enforcement available to them remain far less than the idea of a contract is likely to suggest. The issue of whether prisoners are ahead or behind when they enter a written agreement with prison representatives and parole board members remains problematic despite eighteen months of program implementation during which no prisoner found it worthwhile (and the fundamental question here is whether worthwhile means "necessary" or "safe") to appeal to outside authority to rectify a breach, to challenge a contract term as imposed under duress, or to dispute whether, upon a cancellation of contract, his return to the general prison status had, in fact, been "without prejudice."

The present report is addressed not to the above issues, which are of major importance, but to the lesser questions raised in the American Correctional Association project proposal for Department of Labor funding. These concern generally:

1. The effect of the use of mutual agreement programming upon employability and employment of the inmate following release from prison, including:

   a. length of time from release to first employment
   b. relationship of first and subsequent employment to prison training
   c. changes of employment
   d. salary

2. The effect of mutual agreement programming upon the rate of recidivism. Recidivism for the purposes of this project is defined as any return to prison, including return for the commission of a new felony or for violation of the conditions of parole.

INTRODUCTION

From the early 1960's, and under provisions of the Manpower Development and Training Act, the U. S. Department of Labor supported numerous job and skill training projects for prisoners. While part of a general governmental effort to assist the economically unfortunate classes, added impetus came from basic assumptions of causative association between poverty and criminal activity, and between unemployment and offender recidivism. Faith existed that improvement of skills could enhance social opportunity. By the early 1970's, and in recognition that programs may have instilled skills, but had failed to demonstrate substantial impact on subsequent careers, skepticism increased about both the feasibility of implementation and the conceptual soundness of the underlying model.

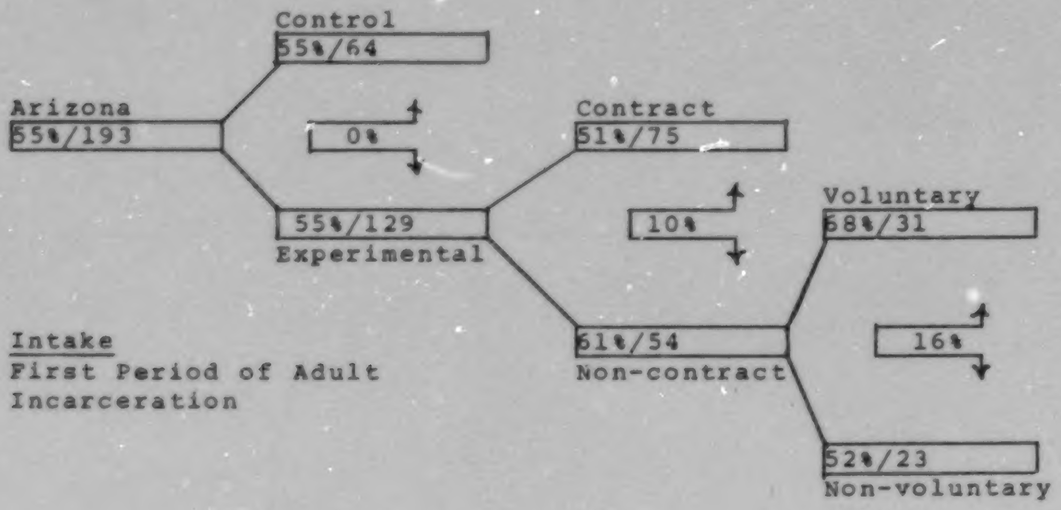
Part of the failure of prisoner training projects was attributed to a failure of coordination, particularly represented as a lag between training completion and prison release during which skills deteriorated and impetus to imvolvement was dissipated. Mutual Agreement Programming--a modification of the prescription programming concept--was advanced as a solution that could reconcile the interests of prisoners, of those responsible for administering training or managing prisons, and those responsible for regulation of confinement period. The new model would retain the prescription program advantage of more certain expectation, while avoiding its one-sided authority implications (doctor-patient) by greater attention to rights and greater assurance of accountability in the form of a legally binding contract. Meanwhile, the relevance of correctional effort--the relative importance of self-improvement to period of punishment and to abatement of subsequent criminal activity or encounters with "the law"--went unquestioned.

The development and refinement of the MAP rationale, and descriptions of its operational implementation in three states under the auspices of the American Correctional Association, are described at length in preceding Resource Documents Numbers 1 through 4 of the Parole-Corrections Project, and will not be treated in detail in the present report.

READER'S GUIDE

Most readers will find this report inordinately long and few will wish to read it in its entirety. The vast majority of pages (from page 37 through page 226) are devoted to a search for evidence of program effects and selection effects in two of the three project states--Wisconsin and Arizona--by means of a rather consistent approach applied to a large number of measures. Owing to the general absence of demonstrable program effects, this form of presentation, which was intended to make it easier for readers to keep their bearings through a great mass of data, instead proves generally tedious and unrewarding for readers. Selective reading is therefore encouraged, and the Table of Contents is sufficiently detailed to help one identify those topics in which he may have particular interest.

Findings are grouped under three major headings which represent data obtained from three different points in time: (1) program intake (page 37); (2) prison release (page 73); community follow-up (page 145). This separation corresponds, generally, to a comparison of prisoners' backgrounds, of their perception of the prison experience, and of their performance subsequent to release, and are generally designed to answer questions about, respectively, the selection process, subjective judgments about the worth of the program, and actual program consequences for future employment and recidivism. The approach, throughout, relies upon a comparison of particular prisoner subgroups, a search for differences among these subgroups on a large number of measures, and a test of the statistical significance of those differences. Findings are typically presented within a standard framework which is depicted in the following diagram:


Control
55%/64
Arizona
55%/193
0%
Contract
51%/75
55%/129
Experimental
10%
Voluntary
68%/31
Intake
First Period of Adult Incarceration
61%/54
Non-contract
16%
52%/23
Non-voluntary

The above example presents information on one index of prior criminal history at the time of assignment to the MAP study sample in Arizona. The diagram is organized to provide three comparisons--experimentals vs. controls, contract experimentals vs. non-contract experimentals, and voluntary non-contract cases vs. non-voluntary non-contract cases. The numbers in each box represent, first, the percent of respondents in each subgroup who were serving their first period of adult incarceration and, second, the number of respondents on which the finding was based. Thus, data were available on this measure for nearly all prisoners in the full 195-member study sample. The first comparison-experimental vs. control-indicates no difference (shown as 0%)and documents that there was no evidence, on this variable, of bias in the randomization procedure that assigned prisoners to these two groups; the importance of such a comparison is to determine whether one group started with an unfair advantage, or whether subsequent performance differences may be safely attributed to program effects. The second comparison--contract vs. non-contract--permits a gross check on possible selection effects in attrition from the contract group. In these comparisons, the "contract" group includes only those experimental cases who actually obtained and completed a contract, while the "non-contract" group contains both cases who entered but then dropped from contract and experimentals who did not enter a contract. Thus, in the example used, cases who completed contracts were about ten percentage points more likely than other experimentals to have a record of prior adult incarceration. The third comparison--voluntary vs. non-voluntary--permits us to better isolate the source of the difference just mentioned: the "voluntary" group contains, primarily, prisoners who declined to enter contracts, plus a few who decided to withdraw from contracts they had entered. It is apparent, from the diagram, that prisoners serving their first adult term were substantially more likely to exercise this option, and that there is no indication that the parole board screened cases from contract participation on the basis of prior record--the "non-voluntary" groups, consisting primarily of prisoners who were denied access to contract plus a few who were subsequently removed from contracts, looks nearly identical on this variable to the group who entered and completed contracts.

In the general approach just described, variables were usually "collapsed" to just two values (eg. "earnings above $3.00/hr," and "earnings below $3.00/hr") and the location of the "cutting point" (eg. at $3.00/hr rather than at $4.00/hr) was generally determined by a so-called median split, or division of the study sample into nearly equal-sized groups. These decisions were based on the desire to keep the presentation as consistent as possible and as concise as possible over the large number of variables, to keep cell frequencies large enough to permit statistical tests, and to guard against post hoc determinations about cutting points that might either unfairly capitalize on maximizing or minimizing differences among comparison groups. In some parts of the report, particularly where a number of measures appear to warrant comparison with one another, a constant threshold is employed across those variables for the purpose of more coherent presentation, rather than a separate cutting point on each measure; even on these occasions, however, the distribution is usually split so that roughly half the responses across measures are above, and half below, the overall cutting point.

Readers may reasonably wonder whether such voluminous data collection and such a repetitive search for differences among comparison groups was warranted, especially in view of the general paucity of findings. In regard to the data collection, it was not as costly a process as it might appear, being primarily handled through two self-administered prisoner questionnaires, each containing about forty items and requiring about one-half hour to complete, supplemented by coding from the actual contract documents and brief follow-up interviews. Under such circumstances, it takes little more effort, once the subjects are present, to collect a great deal of information than it does to collect a small amount. Analysis of so many measures was undertaken primarily as insurance to reduce the chance that some genuine program effect might have been produced which was overlooked, and as a safeguard against mistaking a pre-existing difference or selection effect as a program effect.

Some of the research data collection documents, particularly monthly Status Report Sheets which were devised primarily as a quality control device to assure that information was being acquired on schedule, the Contract Term Sheets, which separated contractual commitments by category to facilitate coding, and Contract Problem Resolution Forms, which provided a vehicle to which other pertinent documents, such as memoranda, could be attached appear to have served the project coordinators by helping them organize material necessary to monitor their daily program operations, while providing them a convenient record of activities. Apart from some of the opinion items in the prisoner questionnaires, these instruments could also be adopted, with permission of the prisoners, to provide a systematic source of information for direct utilization in project operations. The full instrument package has been included as Appendix V of this report for use as a reference source for corrections staff who may be engaged in the conduct of similar projects, and who may wish to adopt some of these materials for their own use. In considering such adaptations, it may also prove helpful on decisions such as whether to include or exclude a particular questionnaire item or to modify its wording for better clarity, to judge it first in terms of whether the information appears to have direct relevance to and possible utility in program operations or management decisions, and then to examine findings involving application of that item in the text of the present report as another form of check on whether it appears suitable for use.

For the reader who is primarily interested in an overview of the research on the MAP demonstration project, it is suggested that a reading of the following sections will probably suffice:

The Research Design Study Sample Assignment Procedures Schedule for Operations	pgs. 7-36
Institution Staff Opinions	pgs. 131-8
Prisoner and Program Subgroups	pgs. 227-46
MAP in California General Summary Conclusions and Recommendations	pgs. 253-77

For the reader who is merely interested in how things turned out, it is advised that the first section ("The Research Design") together with the last two sections ("General

Summary," "Conclusions and Recommendations") may be all that is necessary.

THE RESEARCH DESIGN

OVERVIEW

A classic experimental design was to be employed, with randomized assignment of prisoners to experimental or control status, and negotiations for possible contract entry initiated only with members of the experimental sample. Apart from the advantage for research as a control on introduction of selection bias between the basic comparison groups, it was also the opinion of legal counsel for the Wisconsin corrections agency that, so long as decisions for initial selection into the pool of eligibles were governed by reasonable criteria, and the method of selecting members from that pool as potential program participants was on a purely random and arbitrary basis, "no (legal) action would lie for any inmate not selected."

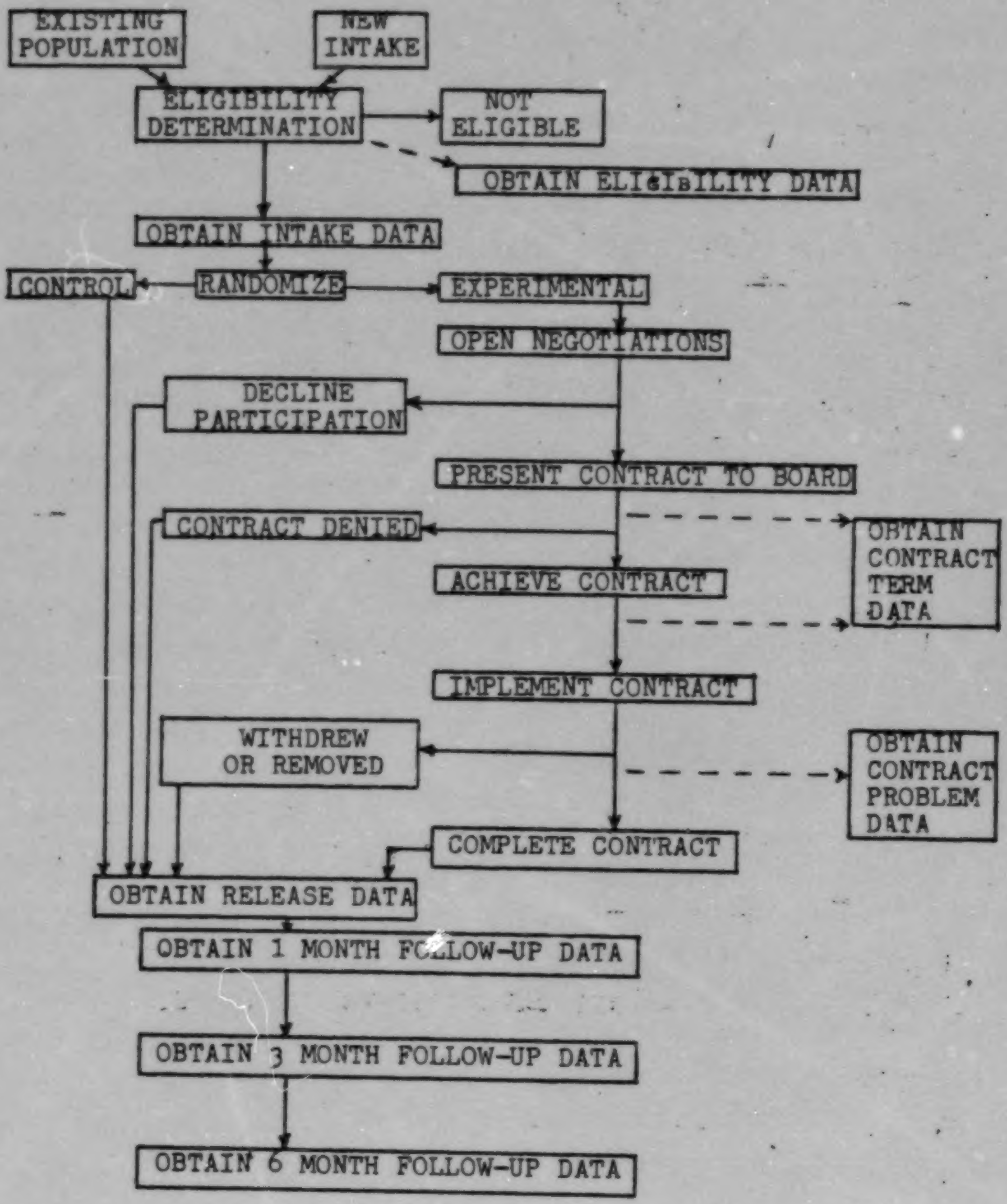
The primary level of comparison was to be between all cases randomized as experimentals, regardless of whether they attained contracts, against all controls. This convention accepts attenuation of the apparent program effects in exchange for assurance that further sampling bias, introduced by negotiation outcomes, will not be reflected in the comparison. Separate analyses were directed at determination of differences existing between experimentals with and without contract, for the purpose of answering questions about program feasibility and generalizability.

OPERATIONAL ASPECTS

The general design is schematized in Figure 1, following. That design was adhered to in Wisconsin and in Arizona, but was aborted in California, where findings are merely descriptive rather than comparative; data collection continued for several months after the termination of the operational program, but then closed because of the loss of an adequate alternative means for continued collection, and because of necessity to schedule the final analysis and report. By the cut-off date, 85% of the full study sample members had been released, and 73% of the full sample was due for six month follow-up; releases were few in the final project months, and many more months could be expected to pass before all the remainder achieved both release and sufficient community exposure period.

GENERAL DESIGN

CASE FLOW AND DATA COLLECTION POINTS


EXISTING POPULATION
NEW INTAKE
ELIGIBILITY DETERMINATION
NOT ELIGIBLE
OBTAIN ELIGIBILITY DATA
OBTAIN INTAKE DATA
CONTROL
RANDOMIZE
EXPERIMENTAL
OPEN NEGOTIATIONS
DECLINE PARTICIPATION
PRESENT CONTRACT TO BOARD
CONTRACT DENIED
OBTAIN CONTRACT TERM DATA
ACHIEVE CONTRACT
IMPLEMENT CONTRACT
WITHDREW OR REMOVED
OBTAIN CONTRACT PROBLEM DATA
COMPLETE CONTRACT
OBTAIN RELEASE DATA
OBTAIN 1 MONTH FOLLOW-UP DATA
OBTAIN 3 MONTH FOLLOW-UP DATA
OBTAIN 6 MONTH FOLLOW-UP DATA

NOTIFICATION OF ELIGIBILITY AND RANDOMIZED ASSIGNMENT

Under the design, once eligibility standards were agreed upon, the state project coordinators supervised the preparation of a list of all prisoners who met criteria. Both a statement of criteria and the list of eligibiles were posted in the institution with an invitation for any prisoner excluded from the list who believed he met the standards to contact the project coordinator, who would review his record. A one page description of the MAP program was to be circulated to the inmate body at this time, together with a brief opinion prepared by the corrections department attorney or the attorney general's office on the legal status of proposed contracts.

Provisions were made to augment the eligible pool from future intake if initially too small, and to winnow it by randomized removals if initially too large. Prisoner representatives witnessed and participated in both the winnowing procedure and the subsequent randomization procedure for designating subjects experimental or control. Between the placement in the final pool as eligibles and randomization to assignment status, each prisoner was administered an intake questionnaire to obtain data on background, beliefs, and plans; testing preceded randomization for the purpose of eliminating differential response biases arising from knowledge of experimental or control status. Since information collected for other pruposes and available in case files is rarely thorough or present in consistent format, and since particular items of information, even if present, may require lengthy search to locate, it was suggested that each eligible prisoner act as his own informant. Statements were accepted at face value for research purposes, with no attempt made to assess their veracity. The same provisions existed for further data collection at the time of release from prison. At the cut-off date for data collection, the few prisoners not yet released were to be administered the release instrument to collect further data on institution programming.

CONTRACT NEGOTIATION AND ENTRY FOR EXPERIMENTALS

This procedure was characterized as involving five sequential phases, with a document representing each phase. These were described as follows:

Contract Term Sheets

These data sheets are identical to page 2 of the actual MAP contract, which provides headings under which to describe the specifics of terms undertaken in the areas of:

1. Skill Training
2. Work Assignment
3. Education
4. Treatment
5. Discipline
6. Other

The contract preparation process is seen as involving five stages, and a separate contract term data sheet is to be prepared at each stage. The basic data forms for all stages are identical, with the stage being identified by checking the appropriate form number in the upper right hand corner of the form.

First, the prisoner is invited to a brief session with the project coordinator for the purpose of considering the possibility of entering a MAP agreement. If interested, the subject is invited to develop his own terms on a form designated as #21. This task should be accomplished in the presence of the project coordinator, and while the coordinator may assist the subject, he is to resist any desire to influence the terms being set down. Form #21 is to represent as closely as possible the subject's own notions of a useful and desirable program.

Second, the project coordinator is to enter actively in discussion with the subject about the feasibility of the agreement being sought, and to make whatever suggestions about terms he believes are appropriate, but deferring to the subject's own judgment if in disagreement. As a result of this discussion, Form #22 is to be completed, representing a tentative program arrived at through collaboration and with input from the project coordinator.

Third, a copy of Form #22 is prepared for the purpose of checking feasibility of the tentative agreement with institution representatives who would be responsible for providing the service to permit satisfaction of specific contract terms. The originals of forms 21 and 22 will, meanwhile, have been stored in the subject's project file. The results of the check on institution feasibility, including term modification, are to be entered on Form #23, which represents a program that the institution is prepared to provide, and indicates the parties responsible for providing it. [The prisoner and the project coordinator may divide the labor involved in determining the opinions of the parties necessary for particular terms. The process may take several days.]

Fourth, the subject and the project coordinator meet for a second session to discuss whatever modifications in the proposed agreement have occurred or been suggested on the basis of institution input, and whether the subject finds these modifications acceptable. At this time, Form #24 is prepared, with the project coordinator being careful to adhere as closely as possible to the subject's wishes, and to not over-ride his objections. Copies of Form #24 are made and submitted to the parole board and to the official institution representative as the contract proposed for negotiation. The originals of forms 23 and 24 will at this time be stored in the Subject's project file.

Fifth, the actual negotiation session will be scheduled, and an effort made to obtain a signed agreement. If the effort is successful, the actual contract term sheet will be copied and used as Form #25, and all five forms (#s 21, 22, 23, 24, 25) will then be forwarded to the research office. If negotiations are unsuccessful, a Form #25 will be prepared to indicate the terms which the parole board and institution sought to impose on the subject. If the process of contract preparation aborts at any of the earlier stages and is not resumed, copies of all term sheets prepared up to that point will be submitted to the research office.

(Note also procedures for recording information from these stages per: instructions for Status Report Sheet.)

CONTRACT IMPLEMENTATION AND COMPLETION

The model stipulated provision of a "date certain" contingent upon performance by the prisoner on contract terms which involved the provision of certain programs and opportunities by the institution. Failure of performance on the part of the institution could affect the ability of the prisoner realistically to undertake or to satisfy certain contract terms, but would not constitute failure on his part and should not have the effect of postponement or forfeiture of the agreed-upon release date.

The model had the project coordinator responsible for monitoring the progress of the prisoner, for written individual monthly progress summaries, and for review with the prisoner and institution and parole officials. In order to keep the burden of reporting within manageable limits, it was suggested that a system of reporting by exception be adopted.

For both operational and research purposes, it was important that contract terms be precise and explicit. For research purposes, at least, it was important that they also be concise. Unfortunately, in the fields of training, education, and counseling or treatment, the states of the arts are generally such that rather heavy reliance must be placed on the judgments of instructors or counselors about a person's level of accomplishment. While it is pleasant to talk about objectively quantifiable, valid and relevant indices of accomplishment, more often than not we find ourselves not in a position to develop or supply them (it is not difficult to obtain measures, but it is difficult to refute a challenge as to the appropriateness of the measure).

As an alternative to making a promise that might be incapable of fulfillment, and as an early alert to misunderstandings that might otherwise develop to crisis proportions, the following compromise was suggested:

Contracts would be worded in the simplest and briefest form possible, preferably one sentence per term. The institution representative primarily responsible for the satisfaction of any given term (e.g., a vocational instructor for a particular training course, or a social worker for group counseling), would be provided a copy of the particular contract term on the appropriate subjects, and called upon to make a monthly judgment of the subject's progress enroute to satisfaction of the term by the proposed date. All judgments were made within the framework of:

40

a. Satisfactory or more than satisfactory; or
b. Less than satisfactory.

The project coordinator was to provide the relevant judge an updated list of the subjects on whom judgments were required, and a schedule indicating dates for judgment on each subject. In the absence of information to the contrary, the project coordinator would enter a rating of "satisfactory" on each contract term for each subject whenever a progress report fell due. It would thus be unnecessary for instructor or social worker (or a designated custodial representative delegated responsibility for disciplinary tracking on project subjects) to submit a monthly judgment on satisfactory or better performances. Only in the event of a rating of "less than satisfactory" was the submission of judgment mandatory, and such submissions were to be supported by a full statement of documentation with regard to the extent of effect on jeopardizing satisfaction of the contract term, and corrective measures that might be necessary if the term was to be satisfied. On any such judgments, it was to be clearly established that the failure resided in the subject's performance rather than resulting from some breach on the part of the program or institution. (For example, if the institution had committed itself to providing a certain number of hours of training or treatment by a given date, and later determined this to be impossible, the prisoner would nevertheless be considered to have discharged his responsibility.) Copies of judgments of "less than satisfactory," including documentation, were supplied to both the prisoner and the project coordinator. The prisoner was to appeal any ratings of "less than satisfactory" if he desired. Even in the presence of "satisfactory" ratings, which assured him of no delay or forfeiture of his parole release date, he might submit written complaints for the contract record if he believed the institution was failing to deliver on program commitments which it had expressed in the contract. The project coordinator was to conduct an inquiry on such complaints, and was required to make written reply on his findings.

The report by exception convention not only saved the project coordinator time and energy in the progress reporting task, but established the burden of proof in a fashion permitting him to focus his attention and effort on resolving problems in contract completions upon notification. When problems were not capable of such resolution, the more formal step of contract renegotiation was necessary.

Information regarding problems in contract implementation was forwarded for research purposes via a system described below:

Whenever a documented rating of less than satisfactory is received, or a complaint from the prisoner, the coordinator will conduct an inquiry, attempt to develop a solution acceptable to all parties, and prepare a written statement describing the steps taken and the outcome obtained on a Contract Problem Resolution Form (Form #31). For any month in which a subject received one or more ratings of less than satisfactory, the number of contract terms in which such ratings were received (i.e., from 1 to 6) will be entered for that subject on the Status Report Sheet, and a copy of both the separate rating documentations by the institution representatives for each term, together with a corresponding Contract Problem Resolution Form for each, will be forwarded to the research office. Written complaints prepared by the prisoner will also be copied and forwarded, together with accompanying CPR Form, but these will not result in any entry on the Status Report Sheet.

The project coordinator should generally alert all parties that problems must be promptly brought to his attention, and that any substantial delay in notifying him will be grounds for discounting or invalidating the seriousness of the problem.

For a concrete example illustrating reconstruction from the research data system of the project career of a single individual--the first to complete contract programing--see the Appendix.

STUDY SAMPLE

ASSIGNMENT PROCEDURES
AND
ATTRITION FROM CONTRACT

CREATING THE STUDY SAMPLE: SIZE AND REPRESENTATIVENESS

As originally conceived, the research design called for a study sample of 200 cases in each state, divided equally into 100 experimentals and 100 controls by a process of randomization. Considering that the project coordinator in each state was to be responsible both for the implementation of the contract program and the collection of all data necessary to research, it appeared unwise to exceed these limits on sample size because of the danger of overburdening the coordinators. There were, however, several other constraints, and several important unknowns that had to be taken into account at the onset of the project, and which had bearing on the final shape of the research design.

First, given the time boundaries of the project funding period, it was necessary to set a deadline by which any contract undertaken would be completed--otherwise, there was the danger that a substantial proportion of contracts might be written for periods sufficiently long to spoil opportunity for follow-up, or even for monitoring by the project coordinator of contract implementation before release. For that reason, we specified that all contracts signed must include release dates prior to the end of September, 1973--a condition that later proved impossible to meet in California because of numerous administrative delays. The deadline for contract terminations exacerbated yet another type of problem: several weeks had to be allowed for the development and confirmation of contract terms for each case if the terms were to be more than superficial and to have reasonable likelihood of being met, and the work-~~load on the~~ project coordinator needed to be spread over time, since he could not develop all contracts simultaneously. With several months alloted for the contract development phase, which was initiated during September, 1972 in Wisconsin, and later in the other two states, it was apparent that no contracts could exceed a year, and that the last ones developed could not exceed a few months. Since the parole boards, although obligated to negotiate in good faith, were under no compulsion to actually enter a contract with every case, since not every offender would be interested in undertaking a contract, and since these rates of withdrawal and rejection could only be guessed at in advance, we were forced to accept that there would either be fewer than 100 experimental cases actually entering a contract, or that the experimental sample would have to be enlarged to allow for slippage. Since there was a strong desire to generate at least 100 signed contracts, the decision was made to enlarge the experimental sample, but to set an upper boundary of 150 in deference to two concerns--exceeding reasonable workload limits for the project coordinator, and the possibility that, without some limit, the parole board might be tempted to reject more cases, knowing that there would be plenty more left to choose from.

There was then, a sequence of partial solutions, with each solution generating some new problem. For example, by this state in the modification of the design, estimates were available which made it apparent there would be an insufficient supply of eligible cases in Wisconsin--a fact which necessitated three additional decisions. First, the Division of Corrections agreed to relax its eligibility standards. In all states, of course, it had been necessary to screen out cases with minimal hope of benefit from the project, including those who would not be statutorily eligible for release during the project period, those under detainer from another jurisdiction, those already in possession of a parole date, and those whose terms were soon due to expire. Wisconsin, additionally, excluded prisoners sentenced under special sex deviation statutes, and had sought also to limit eligibility to offenders with low to moderate maximum sentences, but waived the latter restriction in order to provide more cases. Second, an original stipulation that all cases would come from the existing population at the Wisconsin Correctional Facility in Fox

Lake was amended to permit additional cases to be drawn from new intake for a brief period. Even so, because of the supply problem, and scheduling and workload constraints, it was also decided to alter the experimental-control ratio, yielding 150 experimentals and only 50 controls.

Whereas Wisconsin presented a problem of deficit in supply of subjects, we were faced in Arizona with a surfeit of eligible prisoners, and the problem became one of how to winnow these down to a manageable number. Beginning with the entire population of state prisoners (over 1500) somewhat more than half were eliminated by the screening on statutory eligibility, detainers, parole date in-hand, and imminent discharge. Because we were interested in how generalizable any project findings might be to the total prisoner population, and therefore were motivated to keep exclusion criteria to a minimum, a check was made in Arizona to determine in what ways the eligible group differed from the excluded group, or departed from a representative sample. We found that, as a byproduct of the screening criteria just mentioned, several other differences were introduced. Among the three most sizable commitment offense groups--burglary, robbery, and homicide, only robbers appeared evenly represented in the eligible and ineligible populations. Homicide cases were dramatically under-represented in the eligible pool--about 8% of the total membership, as opposed to 26% among the non-eligibles. To a less marked, but still statistically significant extent, burglars were over-represented among the eligibles. Similarly, statistically significant differences were found on the variables of age, drug usage, and type of admission, with the eligible group containing more new court commitments, more cases with a known history of drug use, and more youthful offenders.

In Arizona, the eligible pool was reduced from over seven hundred to 216 by a random drawing, and then further reduced to 195, who were then randomized in a 2:1 ratio* to experimental or control status. The attrition from 216 to

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*It was believed that Arizona prisoners would be more eager to enter contracts and that the 2:1 ratio (as compared with Wisconsin's 3:1) would provide an adequate supply of potential contract cases.

195 was attributable to two sources of loss--12 were released from prison before it could be determined whether they were interested in contract programming, and 9 others were insufficiently interested to undergo intake testing.

In California, administrative delays and difficulties created a crisis in scheduling and in the supply of eligible which made it necessary to abandon the experimental design. MAP programs were eventually drawn up for a total of 45 prisoners, of which two were transferred to remote prisons before agreements could be signed and another eighteen were refused contracts by the parole board. No control group was established. The bulk of the present report, therefore, deals with implementation of the experimental design in Wisconsin and Arizona; California MAP is treated in a separate section placed toward the end of the report.

COMPARISON GROUPS WITHIN THE STUDY SAMPLE

The research information system was designed to gather data on each member of the study sample as he arrived at several basic gateposts:

I. Intake
II. Release
III. Follow-up

Intake testing was conducted after a subject's admission to the eligible pool, but prior to the randomization which determined his status as an experimental or control. Release testing typically took place a week or so prior to the prisoner's actual release. Follow-up information was obtained at the end of the subject's first, third, and sixth month in the community, ordinarily through an instrument completed by the parole agent on the basis of an interview with his case.

In addition to the above checkpoints, which gathered information on both experimentals and controls, supplementary data were obtained from experimentals at several stages of the contract negotiation procedure, and during each month they were actively under contract.

Data collection was initiated during September, 1972 in Wisconsin and November, 1972 in Arizona, and continued until a cut-off date of June 30, 1974 in Wisconsin, and May 31, 1974 in Arizona.

A standardized form of presentation will be used in this report to depict most findings on the variables examined. This presentation is arranged in terms of comparisons between three successive paired subgroups of the full study sample in each state. These are:

I. Experimental vs. Control
II. Contract Completed vs. Other Experimental
III. Voluntary vs. Non-voluntary Experimental Drop-out

The rationale for these three basic comparisons is as follows:

I. The basic aim of the design is to explore MAP program effects upon institutional career (through release testing) and community adjustment (through follow-up recording). Randomized assignment was initiated to establish equivalent experimental and control subsamples, and comparison on initial status (through intake testing) can establish whether output differences may be contaminated by input differences.

II. While the basic design necessitates comparison of the full experimental sample with the control sample, actual program effects can be imparted to the experimental sample only by those subjects who actually received and completed contracts. Comparison of contract with non-contract experimentals will reveal selection effects (on Intake testing) attributable, in part, to official screening and, in part, to self selection. Subsequent performance differences (release and follow-up) will reflect a combination of treatment and selection effects, although it will not be possible to isolate the separate effects. To the extent that selection effects are evident, comparison between contract experimentals and controls will be inappropriate.

III. Separation of non-contract experimentals into voluntary and non-voluntary subsamples--(Voluntary: those who declined a MAP agreement or withdrew from such an agreement; Non-voluntary: those who were denied a MAP agreement or were removed from such an agreement)--can yield information about factors involved in self-selection vs. those resulting from administrative screening.

CASE FLOW INTO COMPARISON GROUPS

Figure 2 on the following page illustrates the process of case flow, and indicates the number of cases in each subsample. The first branching in each state shows the randomized division into experimental and control samples. The second branching shows that, in Wisconsin, 36 subjects declined MAP involvement at some point during the negotiation process, while 114/150, or 76% accepted involvement and remained candidates for contract until one was either granted or denied them. In Arizona 100/130 experimentals, or 77%, remained in contention for a contract to a point of either receiving it or being ruled out.

Of the experimental cases who did not voluntarily decline MAP agreements, 87/114 in Wisconsin (76%), and 80/100 in Arizona (80%) were officially approved and awarded contracts (third branching). Of those securing a contract, only two individuals in each state subsequently withdrew from MAP programming, so the voluntary non-contract subsamples in both Wisconsin and Arizona consist almost exclusively of "decliners" (fourth branching). Similarly, in Arizona, the non-voluntary subsample is comprised almost entirely of persons rejected from admission to a contract, with only three cases being removed for cause subsequent to entry. In Wisconsin, however, difficulties during the course of contract implementation led to seventeen cancellations of agreement, with nearly all those resulting from disciplinary action (fifth branching). In consequence, only 78% of Wisconsin cases who entered a contract completed it (compared to 94% in Arizona), and the non-voluntary subsample in Wisconsin, therefore, less purely reflects initial screening effects. Further, the burden of imparting a program impact or treatment effect falls upon 68 Wisconsin experimentals out of 150, or 45% while in Arizona, the opportunity for contributing impact is spread over 75* of 130, or 58% of experimentals.

The aim of securing 100 signed (not necessarily completed) contracts in each state was obviously defeated, and the rates of attrition on entry suggest that experimental samples of about 175 in Wisconsin and 160 in Arizona would have been necessary to yield the desired number of signed agreements. There is, however, no way of determining whether the parole board would have become more selective about entering agreements if presented with a larger supply of cases.

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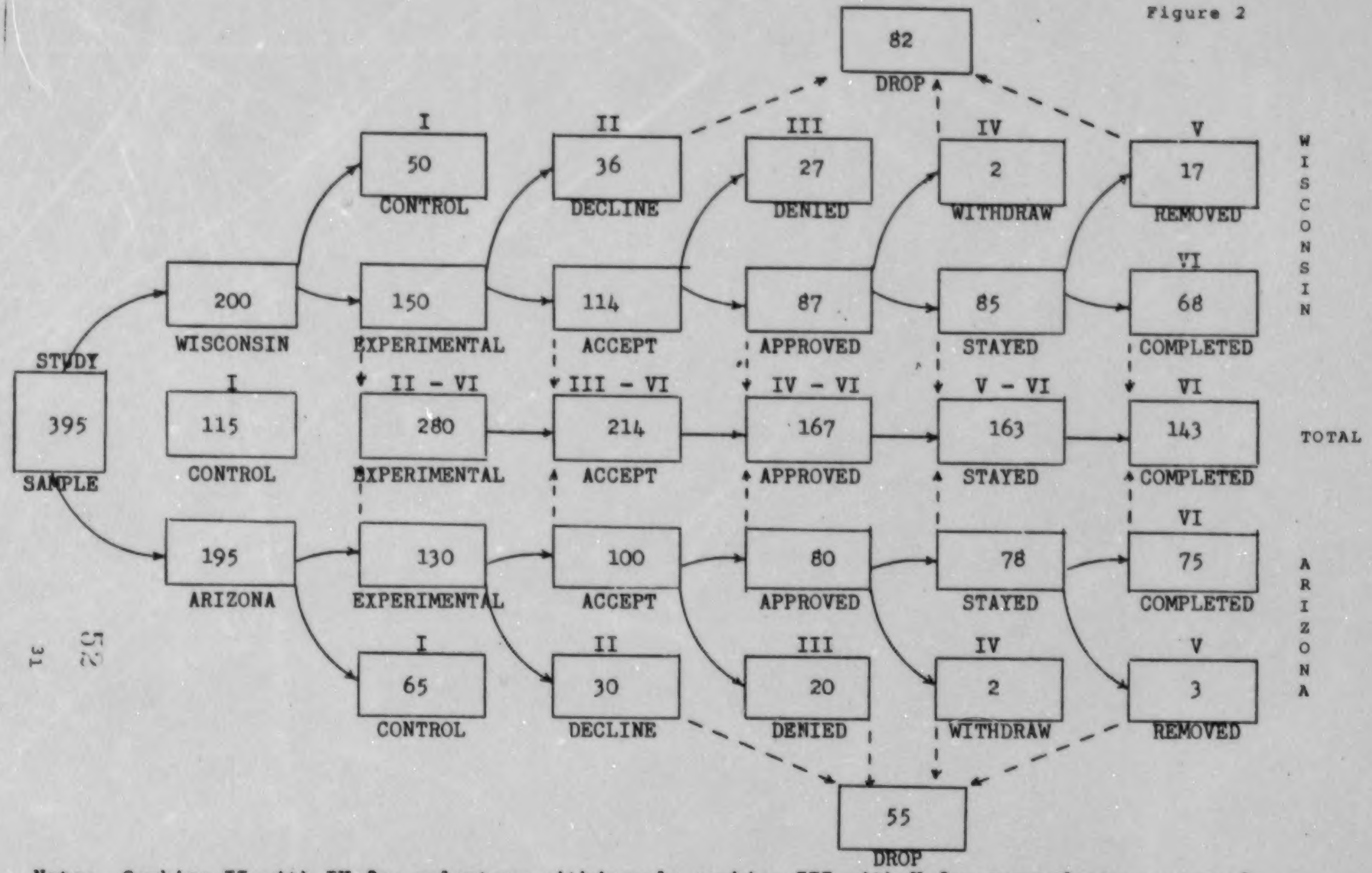
*This figure excludes one prisoner who, though successfully completing contract, was released on detainer to another jurisdiction. In most comparisons in this report, the subsample size will be 76 rather than 75.

MUTUAL AGREEMENT PROGRAM PROJECT CASE FLOW

Figure 2


STUDY
395
SAMPLE
200
WISCONSIN
I
115
CONTROL
195
ARIZONA
I
50
CONTROL
150
EXPERIMENTAL
II - VI
280
EXPERIMENTAL
130
EXPERIMENTAL
I
65
CONTROL
II
36
DECLINE
114
ACCEPT
III - VI
214
ACCEPT
100
ACCEPT
II
30
DECLINE
III
27
DENIED
87
APPROVED
IV - VI
167
APPROVED
80
APPROVED
III
20
DENIED
IV
2
WITHDRAW
85
STAYED
V - VI
163
STAYED
78
STAYED
IV
2
WITHDRAW
V
17
REMOVED
VI
68
COMPLETED
VI
143
COMPLETED
VI
75
COMPLETED
V
3
REMOVED
82
DROP
55
DROP
WISCONSIN
TOTAL
ARIZONA

Note: Combine II with IV for voluntary withdrawal; combine III with V for non-voluntary removal.

Case flow into comparison groups: Wisconsin and Arizona

SCHEDULE FOR OPERATIONS

TIME PERIOD

The random assignment of prisoners from the eligible pool to control or experimental status had taken place during September-October, 1972, in Wisconsin and during November-December, 1972, in Arizona. In Wisconsin, formal contracts were signed with experimental prisoners during the period October, 1972 - February, 1973; in Arizona, the contract signing period was December, 1972 - May, 1973. Since both states were operating with the same target date for fullfillment of contract terms, Arizona contracts tended to be for briefer periods. Further, since the institution facilities and resources devoted to rehabilitation--training and treatment—were more elaborate in Wisconsin, the contracts developed for prisoners there were usually more involved than those in Arizona. The contract negotiation phase occupied five months in Wisconsin, but stretched to seven months in Arizona—interrupted by delay in replacement of a parole board member who retired.

CONTRACT DURATION

In Wisconsin, all 68 prisoners who obtained contracts and did not subsequently lose or forfeit them were released by the end of September, 1973, the date which had originally been specified by which all contracts entered were to be fulfilled. In Arizona, 72 of the 76 prisoners with valid contracts in effect had been released by that date.

Contract periods ranged between one and nine months, with exactly half the prisoners spending four or fewer months under contract before release, and half spending five or more months. The contract periods in Arizona were generally briefer than those in Wisconsin—one-fourth of the Arizona prisoners served contracts of two months or less, whereas there were no contracts shorter than three months in Wisconsin. Two-thirds of Arizona contractees were released within four months of entering contract, compared to one-third in Wisconsin. One-sixth of the cases in Wisconsin spent eight or more months under contract, compared to a single case in Arizona.

TABLE 1

MONTHS FROM CONTRACT ENTRY TO PRISON RELEASE
(Cumulative Distribution)

PERIOD	WISCONSIN	ARIZONA	TOTAL
One month or less	0%	9%	5%
Two months or less	0	24	13
Three months or less	10	50	31
Four months or less	35	62	49
Five months or less	56	71	64
Six months or less	65	82	74
Seven months or less	82	98	91
Eight months or less	96	100	98
Nine months or less	100%	100%	100%

The charts on the pages which follow plot the rates of contract entry and attrition, and prison release from program opening to the target date for last contractual release.

CONTRACT IMPLEMENTATION

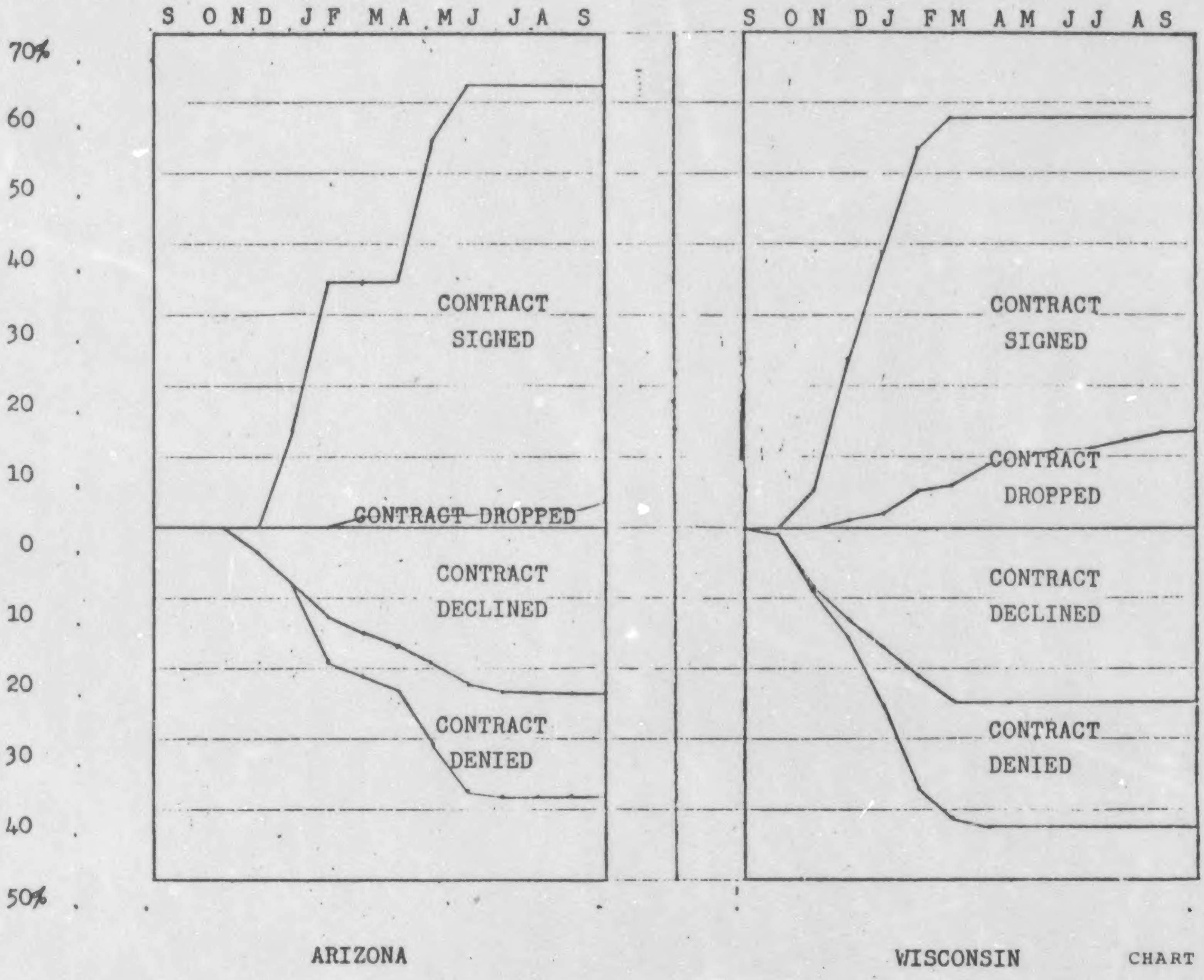
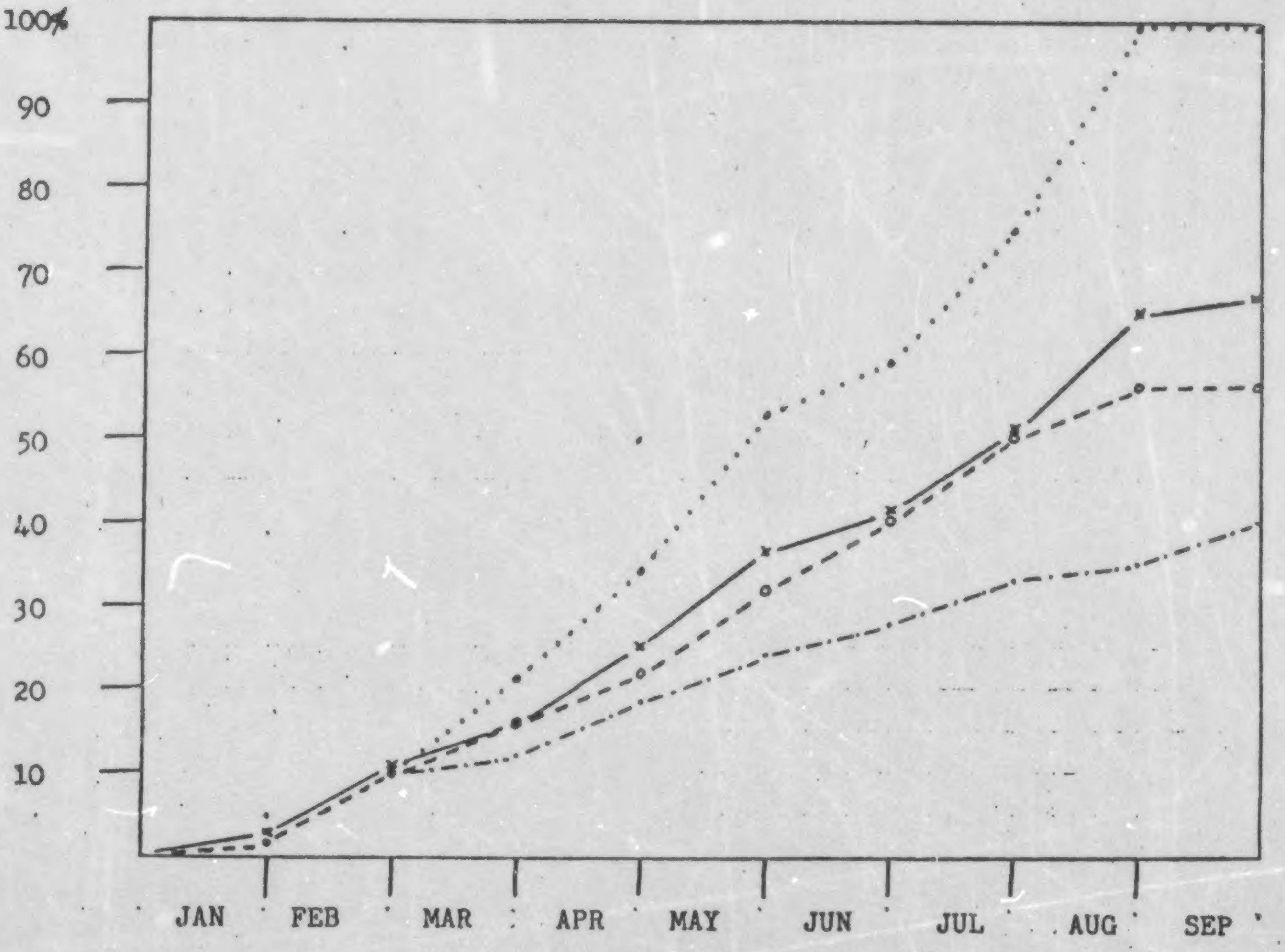


CHART 1

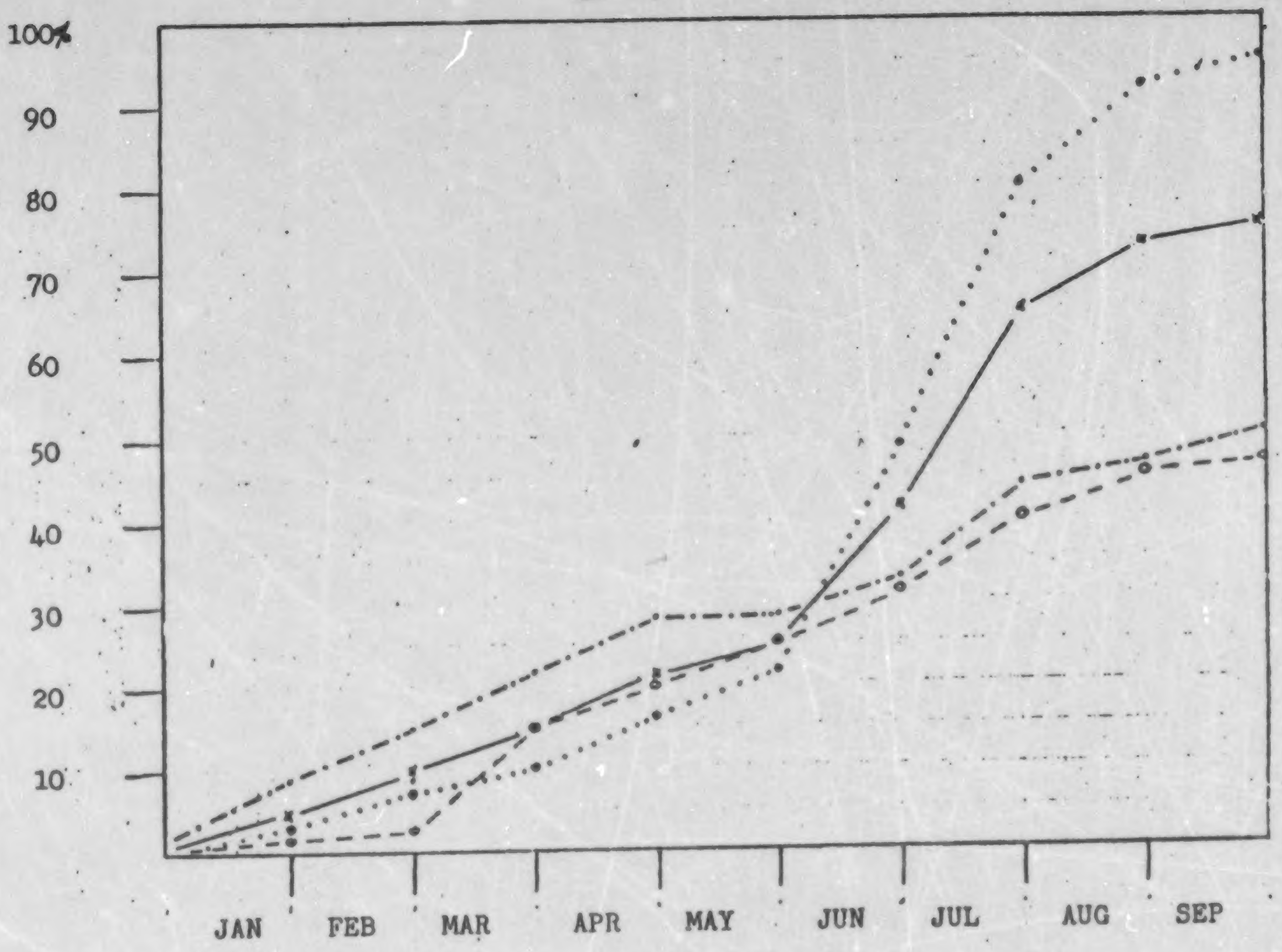


CUMULATIVE RELEASES FROM PRISON: WISCONSIN MAP

Legend

Full Experimental Sample	x———x
Full Control Sample	o— — —o
Experimental Subsample: Contract completed	●.........●
Experimental Subsample: Drop prior or during	.-.-.-.-.-

CHART 2



CUMULATIVE RELEASES FROM PRISON: ARIZONA MAP

Legend

Full Experimental Sample x————x
Full Control Sample o————o
Experimental Subsample: Contract completed ●........●
Experimental Subsample: Drop prior or during .-.-.-.-

CHART 3

INITIAL STATUS
AND
DIFFERENCES AMONG COMPARISON GROUPS

COMMITMENT OFFENSE

WISCONSIN

Nearly one-third (31%) of the members of the Wisconsin study sample were serving sentences for burglary; robbery (22%) and forgery (11%) were the only two other offense categories which contained more than 10% of the study sample. For purposes of comparison, specific offenses were grouped into three larger offense categories:

PROPERTY Burglary, Forgery, Theft, Auto Theft
VIOLENCE OR THREAT: Homicide, Assault, Robbery
SEX OR DRUGS: Rape*, Sex Perversion, Narcotics, Dangerous Drugs, Marijuana

The composition of experimental and control samples is quite similar, differing by 5 percentage points on property offenses, by 6 on six or drug offenses, and by 13 on violence or threat offenses. The differences are not statistically significant, either by overall test of a 2 x 3 table (two conditions by three offense categories) or by a subtest of violence vs. other. Comparisons between experimental subsamples--contract vs. non-contract, and voluntary vs. nonvoluntary drops--indicate no evidence of commitment offense operating as a selection factor in Wisconsin. (See following Tables 12 a, b, c.)

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*Subjects had not been asked to distinguish whether their commitment was for forcible or statutory rape.

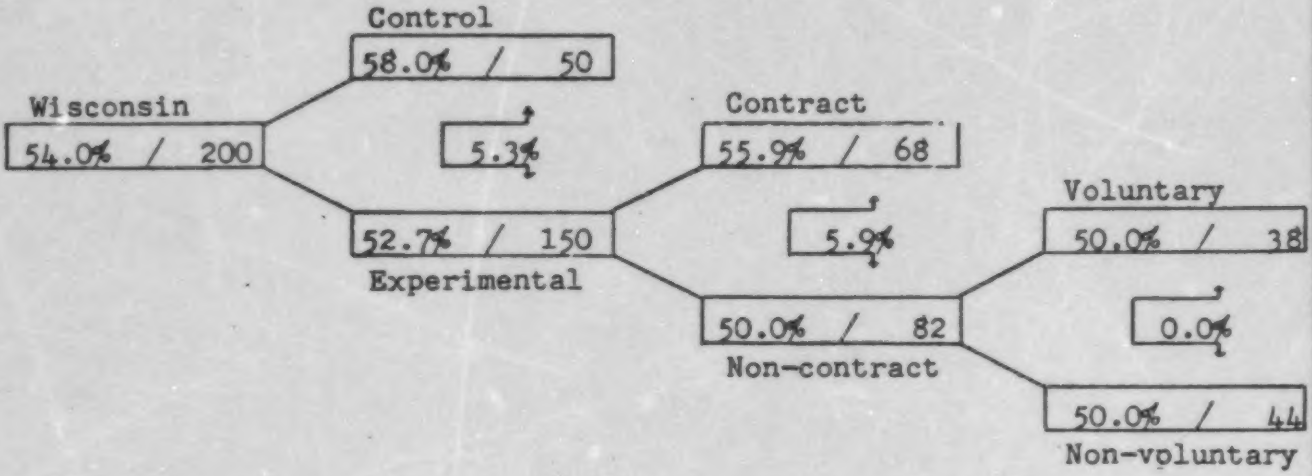
ARIZONA

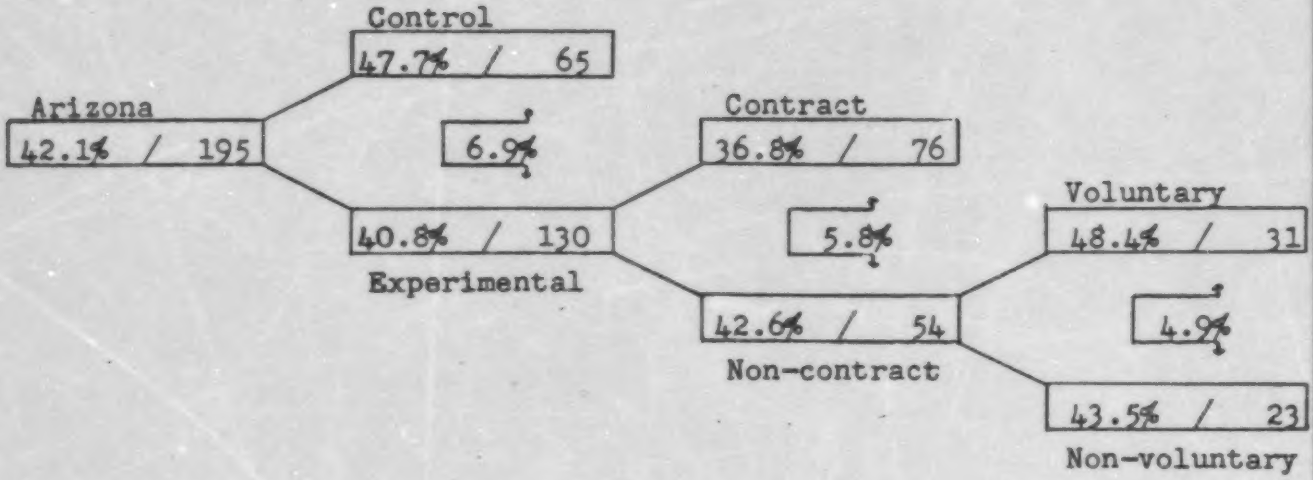
Burglary (20%) and robbery (16%) were the most frequent commitment offenses among members of the Arizona study sample, with assault, theft, and drugs each also accounting for at least 10% of the total.

Camparison in terms of the same major offense groupings applied in Wisconsin again reveals no significant differences between experimentals and controls, although all differences are in the same direction--over-representation among experimentals on violence or threat, under-representation on property, and on sex or drugs--and the largest difference (12 percentage points) is again found on offenses involving threat or violence.

Comparison between contract and non-contract experimentals in Arizona reveals a selection effect to be operative, with X^2 test of the 2 x 3 table yielding a value of 8.90 (2 df; $p < .025$). Contract cases are substantially over-represented on violence or threat offenses (21 percentage point difference) and substantially under-represented on sex or drug offenses (18 percentage point difference). Non-voluntary removals are less like contract cases than voluntary withdrawals in both these major categories, suggesting that the differences result more from administrative screening than from self-selection, with sex or drug cases screened out of, and violence or threat cases screened into the contract group. A more focussed examination reveals that sex perversion cases were most vulnerable to out-screening, with five of the six contract applicants in this offense category being rejected or removed. (See following Tables 12 a, b, c.)

1 INTAKE
PRISON COMMITMENT OFFENSE WAS PROPERTY TYPE
[Burglary, Forgery, Theft, Auto Theft]

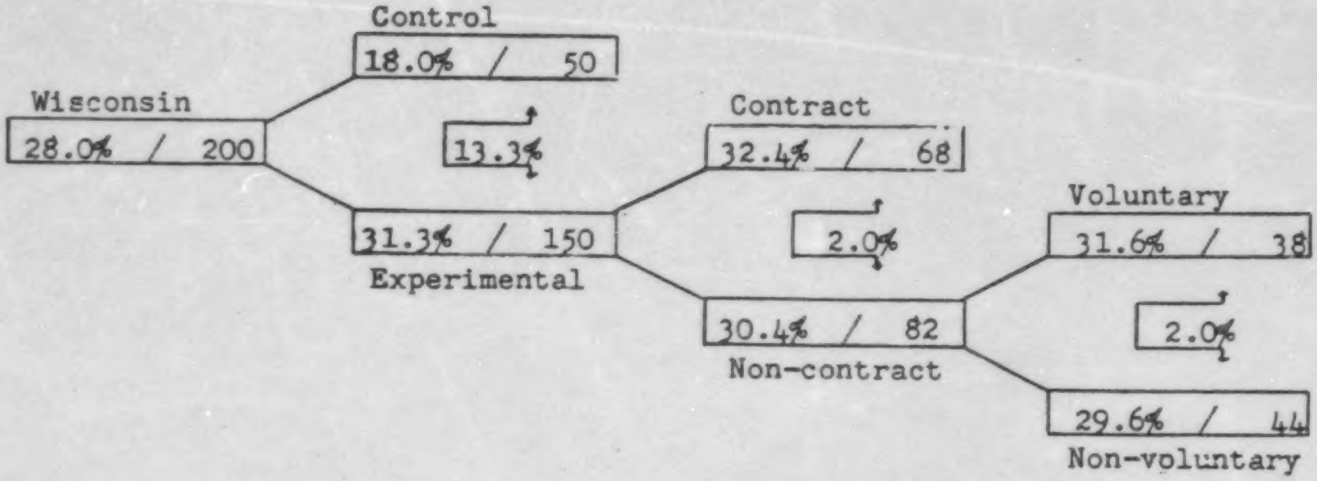

Control
58.0% / 50
Wisconsin
54.0% / 200
5.3%
Contract
55.9% / 68
52.7% / 150
Experimental
5.9%
Voluntary
50.0% / 38
50.0% / 82
Non-contract
0.0%
50.0% / 44
Non-voluntary

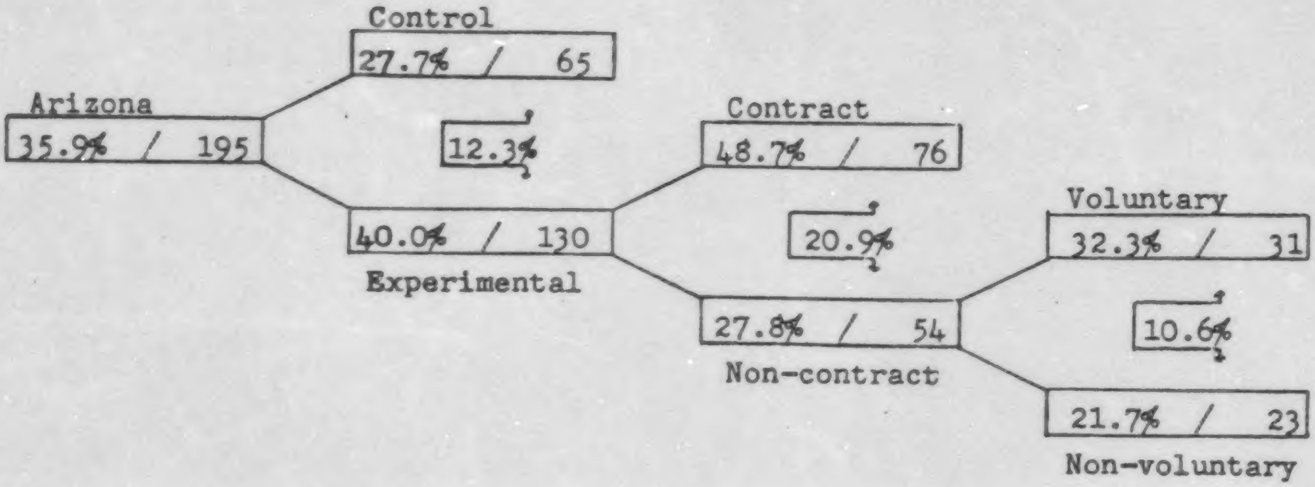

Control
47.7% / 65
Arizona
42.1% / 195
6.9%
Contract
36.8% / 76
40.8% / 130
Experimental
5.8%
Voluntary
48.4% / 31
42.6% / 54
Non-contract
4.9%
43.5% / 23
Non-voluntary

2 INTAKE

PRISON COMMITMENT OFFENSE INVOLVED VIOLENCE OR THREAT

[Homicide, Assault, Robbery]


Wisconsin
28.0% / 200
Control
18.0% / 50
13.3%
31.3% / 150
Experimental
Contract
32.4% / 68
2.0%
30.4% / 82
Non-contract
Voluntary
31.6% / 38
2.0%
29.6% / 44
Non-voluntary


Arizona
35.9% / 195
Control
27.7% / 65
12.3%
40.0% / 130
Experimental
Contract
48.7% / 76
20.9%
27.8% / 54
Non-contract
Voluntary
32.3% / 31
10.6%
21.7% / 23
Non-voluntary

3 INTAKE

PRISON COMMITMENT OFFENSE INVOLVED SEX OR DRUGS

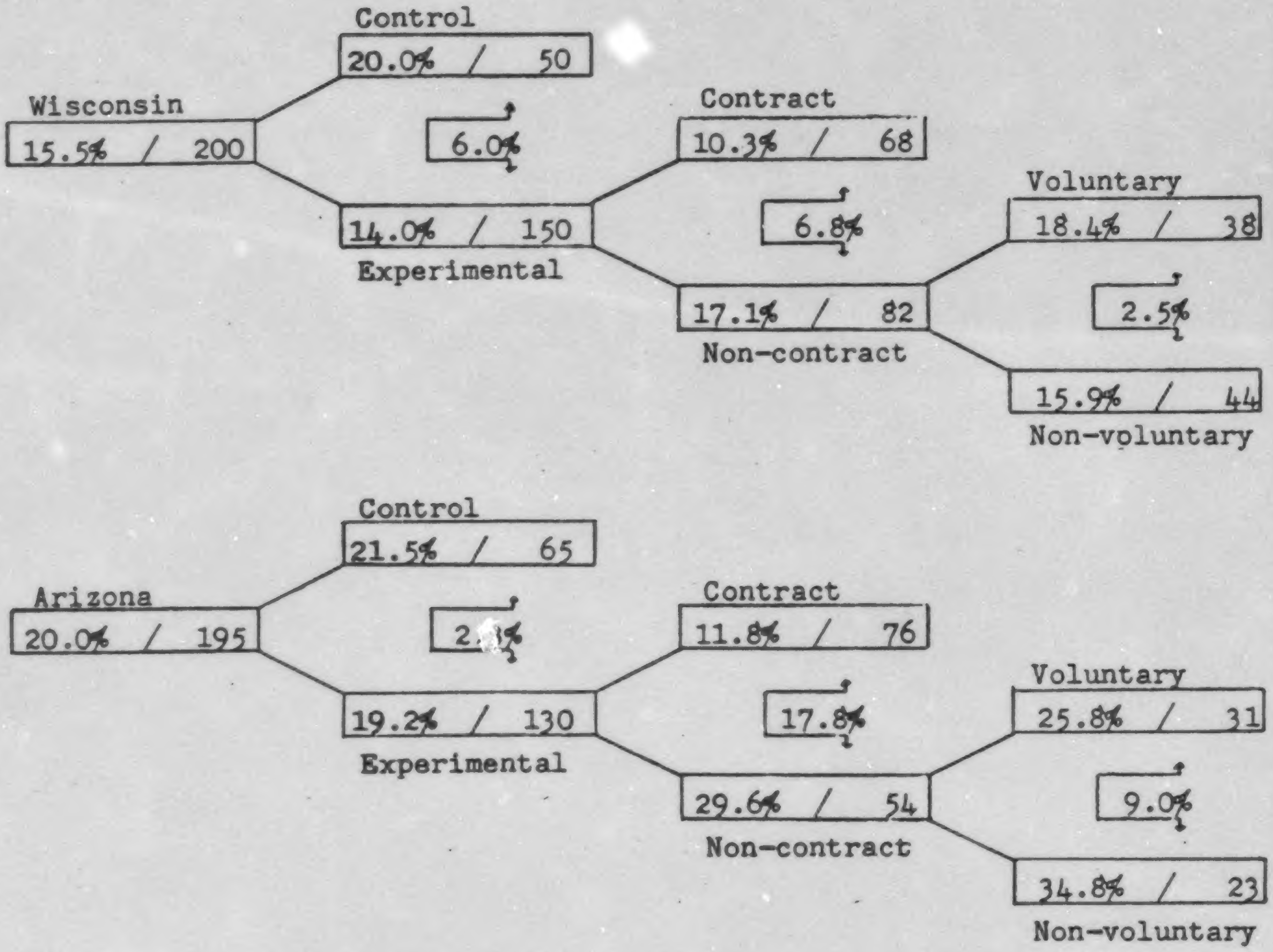

Control
20.0% / 50
Wisconsin
15.5% / 200
6.0%
Contract
10.3% / 68
14.0% / 150
Experimental
6.8%
Voluntary
18.4% / 38
17.1% / 82
Non-contract
2.5%
15.9% / 44
Non-voluntary
Control
21.5% / 65
Arizona
20.0% / 195
2.3%
Contract
11.8% / 76
19.2% / 130
Experimental
17.8%
Voluntary
25.8% / 31
29.6% / 54
Non-contract
9.0%
34.8% / 23
Non-voluntary

TABLE 2

COMMITMENT OFFENSE BY STUDY SAMPLE GROUP

WISCONSIN							ARIZONA					
C	V	NV	Ct	X	SS	COMMITMENT OFFENSE	C	V	NV	Ct	X	SS
29	19	22	38	79	108	PROPERTY	31	13	10	28	53	82
12	12	16	23	51	63	Burglary	13	8	5	14	27	40
5	1	3	3	7	12	Theft	14	3	2	5	10	24
7	2	3	10	15	22	Forgery	2	1	2	5	9	10
5	4	0	2	6	11	Auto Theft	2	1	1	4	7	8
9	12	13	22	47	56	VIOLENCE OR THREAT	18	10	5	37	52	70
6	9	11	19	39	45	Robbery	5	5	3	18	26	31
2	2	1	3	6	8	Assault	10	3	1	12	16	26
1	1	1	0	2	3	Homicide	3	2	1	7	10	13
5	3	5	3	11	16	SEX	6	3	7	2	12	18
4	2	3	1	6	10	Rape, inc. Stat.	3	1	2	1	4	7
1	1	2	2	5	6	Other Sex	3	2	5	1	8	11
5	4	2	4	10	15	DRUGS	8	5	1	7	13	21
2	0	2	1	3	5	MISCELLANEOUS	2	0	0	2	2	4
					200	TOTAL						195

Study Sample Groups

C = Control
V = Voluntary Drops from Contract (Experimental)
NV = Non-voluntary Drops from Contract (Experimental)
Ct = Contract Experimental
X = Total Experimental
SS = Total Study Sample

PREVIOUS ADULT PRISON INCARCERATION

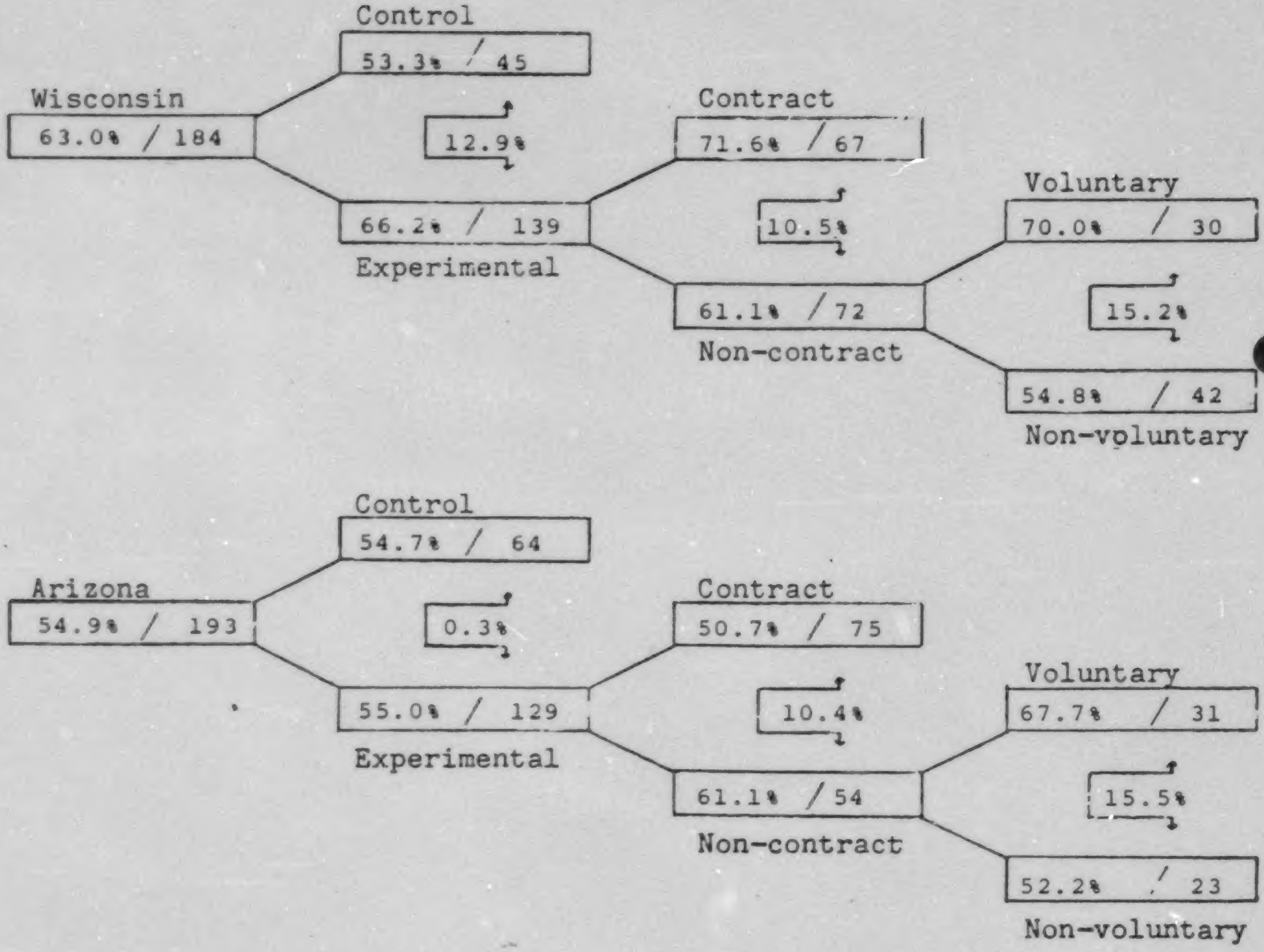
WISCONSIN

Three-fifths (63%) of the Wisconsin study sample members were serving their first period of prison confinement as an adult. Controls were 13 percentage points more likely than experimentals to have experienced a past term of incarceration. Non-contract experimentals were 10 percentage points less likely than those who completed contracts to be serving their first adult prison term, and this idfference appears primarily attributable to a parole board screening effect during contract decisions.

ARIZONA

Somewhat more than half (55%) of Arizona subjects had no record, other than the current commitment, of past adult prison incarceration, and control and experimental samples were almost identical on this measure. Whereas in Wisconsin non-contract experimentals were more likely than contract cases to have a prior confinement record, they were in Arizona somewhat less (10 percentage points)likely than contract cases to have such a record; this difference appears, in Arizona, attributable to a self-selection effect, with first offenders more likely to remove themselves from contract consideration.

4 INTAKE
FIRST PERIOD OF ADULT PRISON INCARCERATION


Control
53.3% / 45
Wisconsin
63.0% / 184
12.9%
Contract
71.6% / 67
66.2% / 139
Experimental
10.5%
Voluntary
70.0% / 30
61.1% / 72
Non-contract
15.2%
54.8% / 42
Non-voluntary
Control
54.7% / 64
Arizona
54.9% / 193
0.3%
Contract
50.7% / 75
55.0% / 129
Experimental
10.4%
Voluntary
67.7% / 31
61.1% / 54
Non-contract
15.5%
52.2% / 23
Non-voluntary

TIME SERVED IN PRISON PRIOR TO RANDOMIZATION

WISCONSIN

In Wisconsin, only 14% of the members of the study sample had served more than two years in prison from last admission to assignment to the MAP eligible pool, and the median length of stay was 10 months. About 10% more of the experimentals than controls had served ten or fewer months--a non-significant difference--and differences among the experimental subsamples were quite small.

ARIZONA

About one-quarter (26%) of the Arizona study sample had spent two or more years in prison on their current stay prior to designation as MAP eligibles, and the median length of stay was 15 months. A 19 percentage point difference exists between the experimentals and controls, with the former significantly more likely ($X^2 = 5.67$; $p < .025$) to have served more than the median period. While such a difference could be expected to arise, through chance, about once in forty times, and while no source has been isolated to which this "failure" in randomization can be attributed, the consequences of the difference between experimentals and controls on this variable are found to be serious for other comparisons treated later in this report.

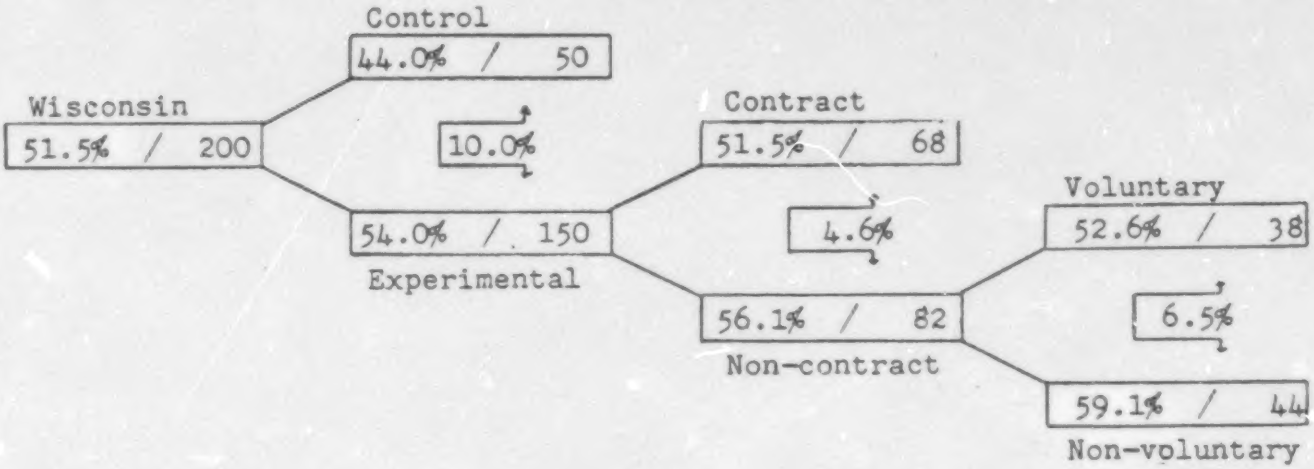
Non-voluntary contract drops were slightly (5 percentage points) more likely than voluntary drops to have served greater than the median period, and non-contract cases more likely (10 points) than those who completed contract to have accumulated more time in prison at the time MAP was initiated.

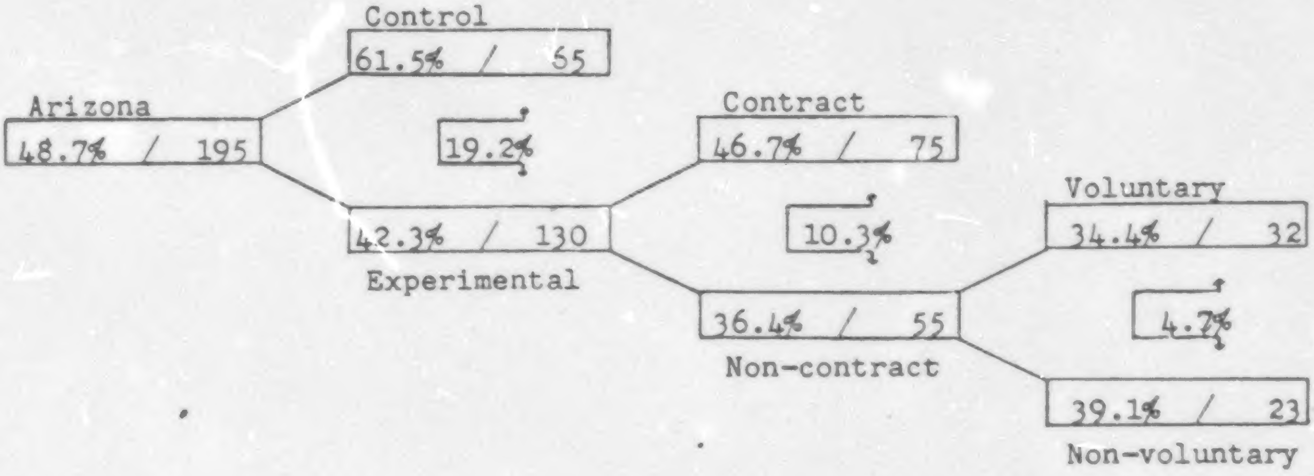
5 INTAKE

MEDIAN TIME SERVED BEFORE MAP RANDOMIZATION

Wisconsin: Served 10 or fewer months

Arizona: Served 14 or fewer months


Wisconsin
51.5% / 200
Control
44.0% / 50
10.0%
54.0% / 150
Experimental
Contract
51.5% / 68
4.6%
56.1% / 82
Non-contract
Voluntary
52.6% / 38
6.5%
59.1% / 44
Non-voluntary


Arizona
48.7% / 195
Control
61.5% / 65
19.2%
42.3% / 130
Experimental
Contract
46.7% / 75
10.3%
36.4% / 55
Non-contract
Voluntary
34.4% / 32
4.7%
39.1% / 23
Non-voluntary

CURRENT AGE AT TIME OF MAP PROJECT ENTRY

WISCONSIN AND ARIZONA

Negligible differences in median age were found in both Wisconsin and Arizona in the comparisons between experimentals and controls, and between contract and non-contract experimentals. In each state, a fairly sizable (17-18 percentage points), although not statistically significant difference existed between subjects who were denied or removed from contract programming, and those who declined or voluntarily withdrew from participation, with voluntary drops tending to be younger than contract experimentals, and non-voluntary drops tending to be older.

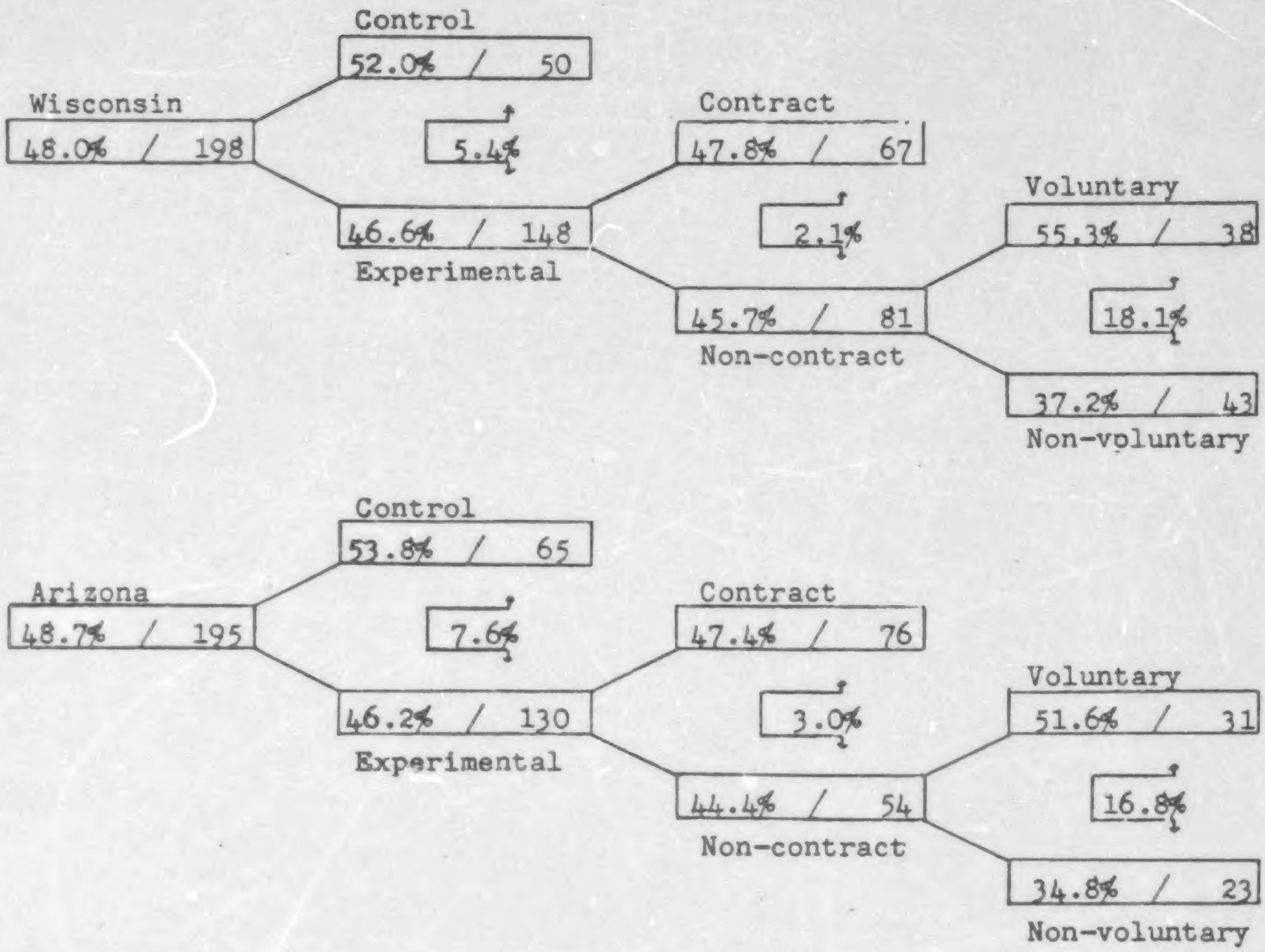
The median age of members of the study sample was 24 in Wisconsin, and 29 in Arizona. Ages in Wisconsin ranged from 18 to 52, with the oldest recipient of a contract being 49; in Arizona, the age range was 17-52, and two 52-year-olds were awarded contracts.

6 INTAKE

MEDIAN AGE AT TIME OF MAP INTAKE

Wisconsin: 23 Years or Less

Arizona: 28 Years or Less


Control
52.0% / 50
Wisconsin
48.0% / 198
5.4%
Contract
47.8% / 67
46.6% / 148
Experimental
2.1%
Voluntary
55.3% / 38
45.7% / 81
Non-contract
18.1%
37.2% / 43
Non-voluntary
Control
53.8% / 65
Arizona
48.7% / 195
7.6%
Contract
47.4% / 76
46.2% / 130
Experimental
3.0%
Voluntary
51.6% / 31
44.4% / 54
Non-contract
16.8%
34.8% / 23
Non-voluntary

ETHNIC STATUS OF THE STUDY SAMPLE

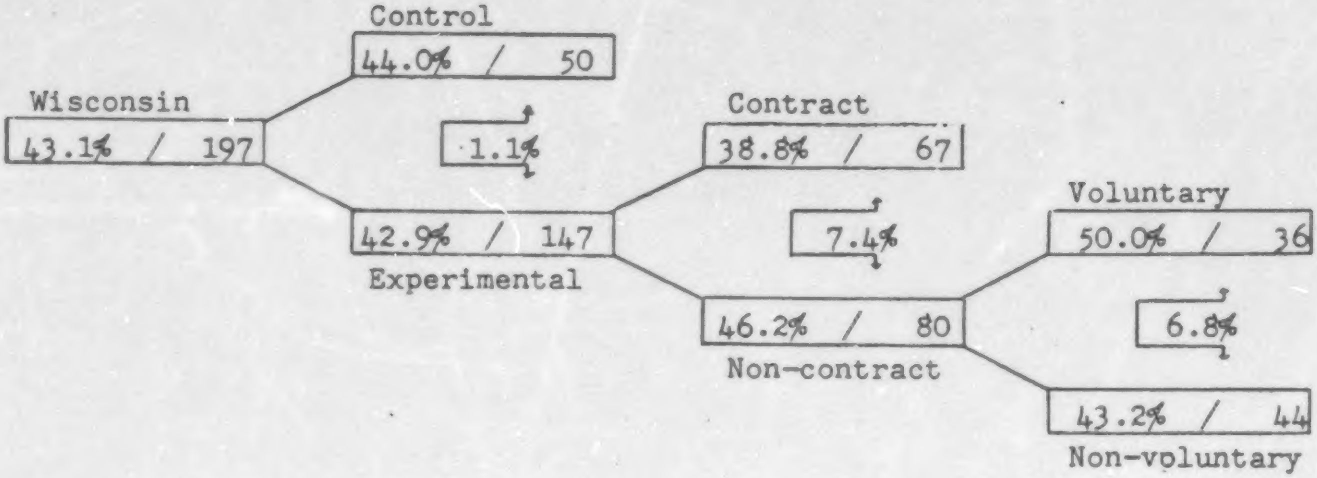
WISCONSIN AND ARIZONA

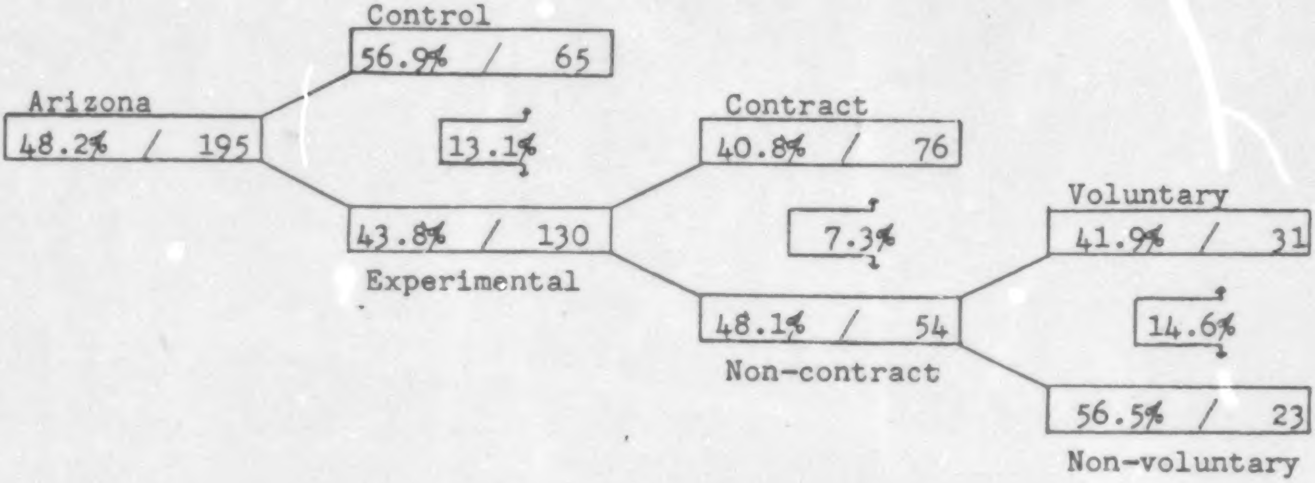
Ethnic minority group members constituted 43% of the Wisconsin study sample and 48% of the Arizona study sample. In Wisconsin, minority representation was predominately Black, and in Arizona, nearly balanced between Black and Chicano, with Native Americans numbering only 6 and 7 members in the two study samples.

There was essentially no difference (1 percentage point) in minority representation between experimentals and controls in Wisconsin. In Arizona, members of ethnic minority groups were over-represented in the control sample (13 percentage points higher than experimentals) but the difference is not statistically significant ($X^2 = 2.47; p < .25$).

Only slight (7 percentage point) differences were found between contract and non-contract experimental samples in each of the two states. Persons denied or non-voluntarily removed from Mutual Agreement Programming in Arizona appear more likely than those completing, declining, or voluntarily withdrawing from contracts to be from minority groups, but the differences (almost 15 percentage points) are in neither case statistically significant.

7 INTAKE
MEMBER OF MINORITY ETHNIC GROUP


Wisconsin
43.1% / 197
Control
44.0% / 50
1.1%
42.9% / 147
Experimental
Contract
38.8% / 67
7.4%
46.2% / 80
Non-contract
Voluntary
50.0% / 36
6.8%
43.2% / 44
Non-voluntary


Arizona
48.2% / 195
Control
56.9% / 65
13.1%
43.8% / 130
Experimental
Contract
40.8% / 76
7.3%
48.1% / 54
Non-contract
Voluntary
41.9% / 31
14.6%
56.5% / 23
Non-voluntary

EDUCATIONAL BACKGROUND AT TIME OF PROJECT ENTRY

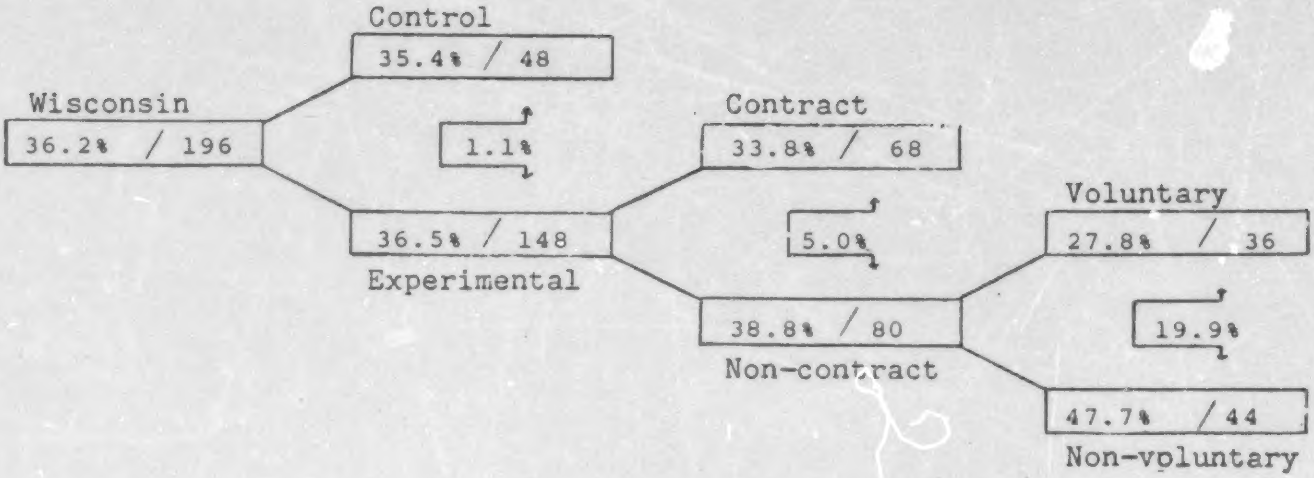
WISCONSIN

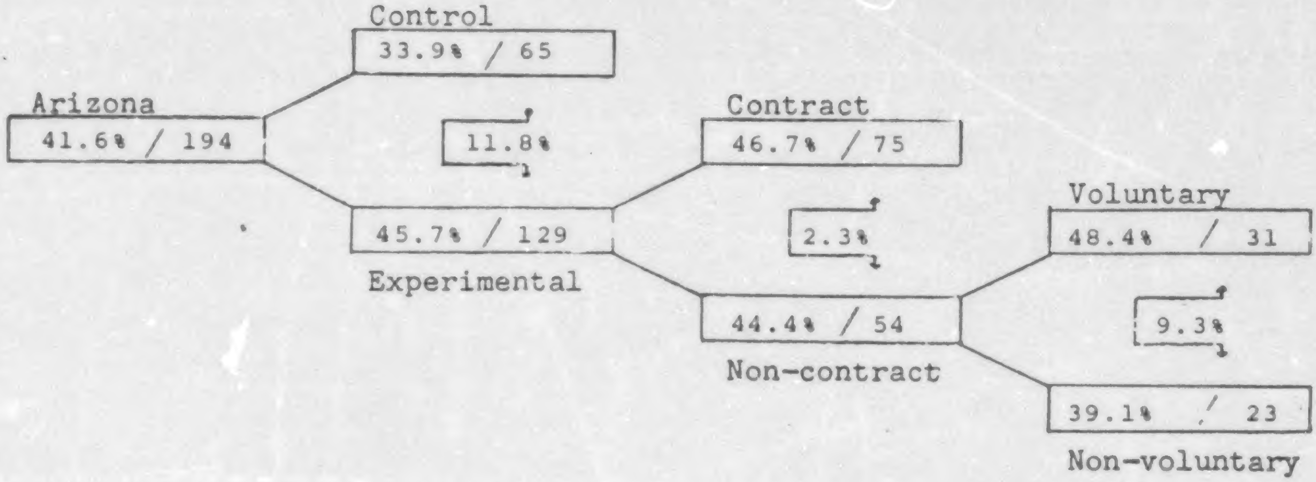
At the time of assignment to the MAP study sample, 36% of Wisconsin cases claimed completion of a high school education or GED equivalent, and control and experimental samples were nearly identical (1 percentage point difference). Non-contract experimentals were 5 percentage points more likely than contract cases to have finished high school, and, among the non-contract subsamples, voluntary drops were substantially less likely (20 percentage points) than non-voluntary drops to have completed twelve grades schooling. This last difference is not statistically significant; since, as compared to the contract sample, the nature of difference consists primarily of an elevation within the non-voluntary subsample rather than a decrement in the voluntary subsample, a screening effect is suggested (i.e., the parole board and/or institution being less likely to accept more educated subjects into Mutual Agreement Programming).

ARIZONA

Forty-two percent of Arizona subjects claimed completion of 12 grades schooling. A 12 percentage point advantage existed for the experimentals, as compared to controls, but was not found to be statistically significant. Differences among the remaining comparison groups were slight.

8 INTAKE
HIGH SCHOOL EDUCATION COMPLETED


Control
35.4% / 48
Wisconsin
36.2% / 196
1.1%
Contract
33.8% / 68
36.5% / 148
Experimental
5.0%
Voluntary
27.8% / 36
38.8% / 80
Non-contract
19.9%
47.7% / 44
Non-voluntary


Control
33.9% / 65
Arizona
41.6% / 194
11.8%
Contract
46.7% / 75
45.7% / 129
Experimental
2.3%
Voluntary
48.4% / 31
44.4% / 54
Non-contract
9.3%
39.1% / 23
Non-voluntary

BEST HOURLY WAGE PRECEDING INCARCERATION

WISCONSIN

In Wisconsin, exactly three-fifths of subjects claimed they had, at some time prior to their incarceration, held a job which paid $3.00 or more per hour. Subjects attaining this earning level were over-represented by 10% in the experimental, as contrasted to the control sample, but the difference is not statistically significant. Similarly, a 15 percentage point difference between contract and non-contract experimentals, with the latter more likely to have reached the higher earning level, is not significant.

ARIZONA

In Arizona, less than two fifths of subjects had achieved an earning level of $3.00/hour or more during the year immediately preceding incarceration.* No substantial differences were found among any of the comparison groups.

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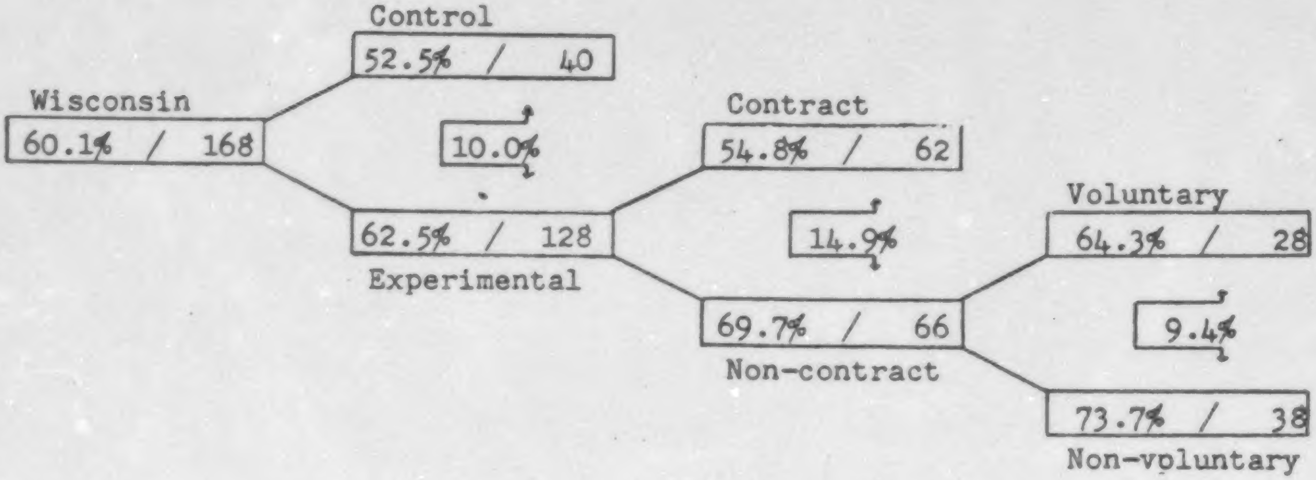
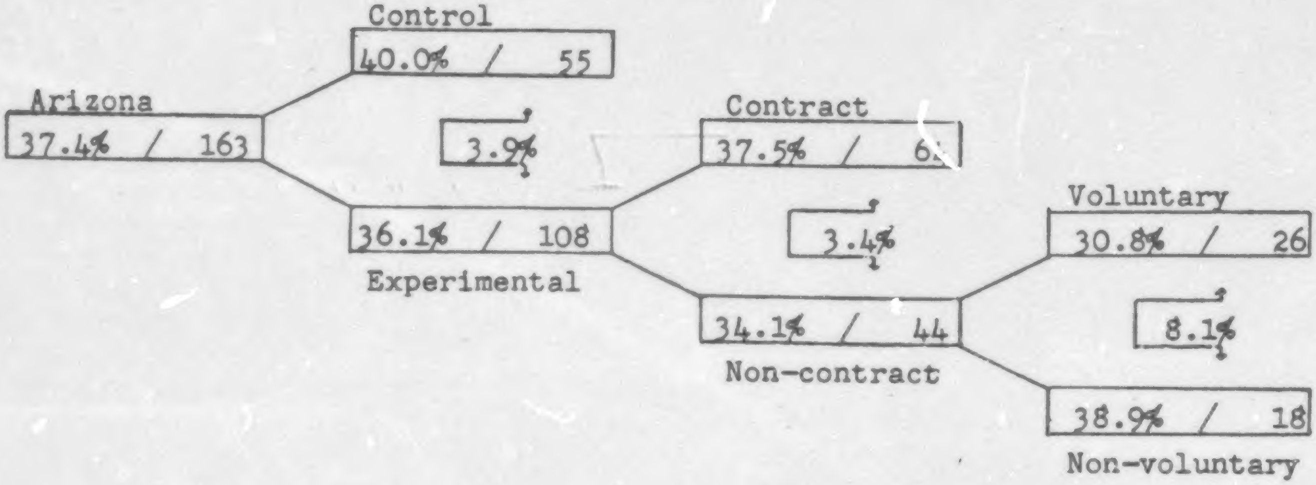
*Intake Questionnaire item wording on items dealing with prior occupation differs for the two states; Wisconsin subjects were asked about the best job ever held, and Arizona subjects about the best job during the last year before their admission to prison.

9 INTAKE
BEST JOB PRIOR TO INCARCERATION PAID $3.00/HR. OR MORE

[Item wording differs between states]


Wisconsin
60.1% / 168
Control
52.5% / 40
10.0%
62.5% / 128
Experimental
Contract
54.8% / 62
14.9%
69.7% / 66
Non-contract
Voluntary
64.3% / 28
9.4%
73.7% / 38
Non-voluntary

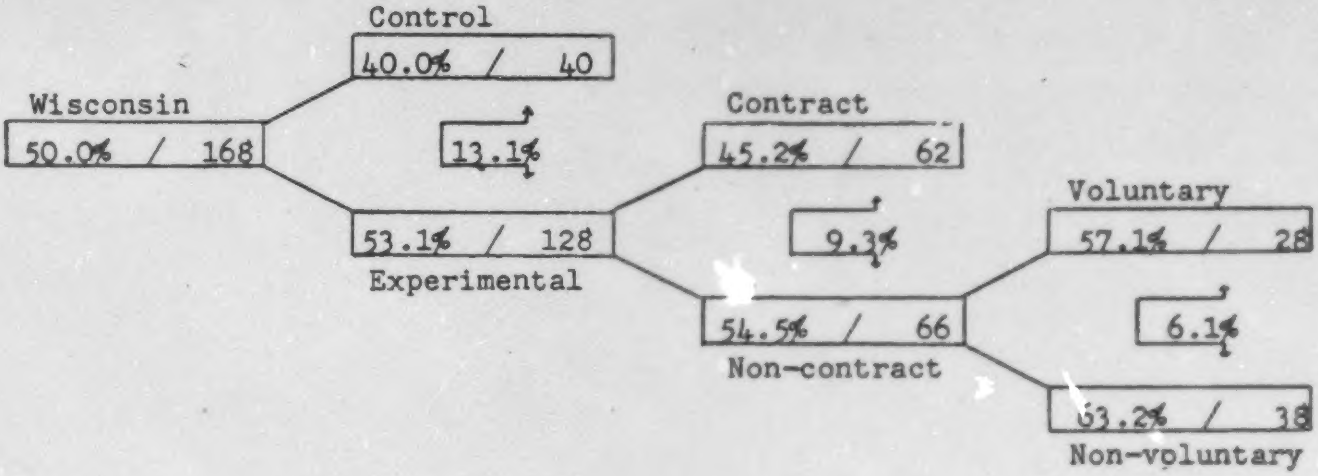

Arizona
37.4% / 163
Control
40.0% / 55
3.9%
36.1% / 108
Experimental
Contract
37.5% / 6
3.4%
34.1% / 44
Non-contract
Voluntary
30.8% / 26
8.1%
38.9% / 18
Non-voluntary

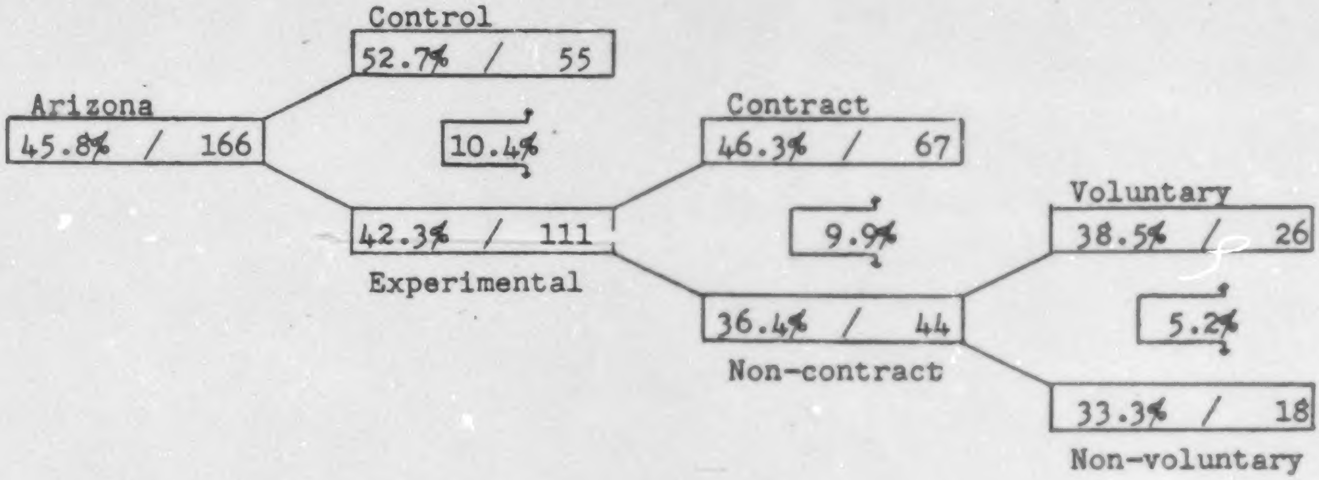
MONTHLY TAKE-HOME PAY PRECEDING INCARCERATION

WISCONSIN AND ARIZONA

Wisconsin experimentals were more likely (13 percentage points) than controls to have reached an earning level yielding more than $400 per month at some time in their past, and non-contract experimentals more likely (9 percentage points) than those with contracts to have reached that level. In Arizona, the direction of differences among comparison groups was reversed, with controls and contract experimentals achieving higher earning levels in the year preceding incarceration. These directional differences on monthly earnings parallel those on the hourly wage measure, but none of the differences reaches a statistically significant level.

10 INTAKE
BEST JOB PRIOR TO INCARCERATION PAID $400/MO. OR MORE
[Item wording differs between states]


Wisconsin
50.0% / 168
Control
40.0% / 40
13.1%
53.1% / 128
Experimental
Contract
45.2% / 62
9.3%
54.5% / 66
Non-contract
Voluntary
57.1% / 28
6.1%
63.2% / 38
Non-voluntary

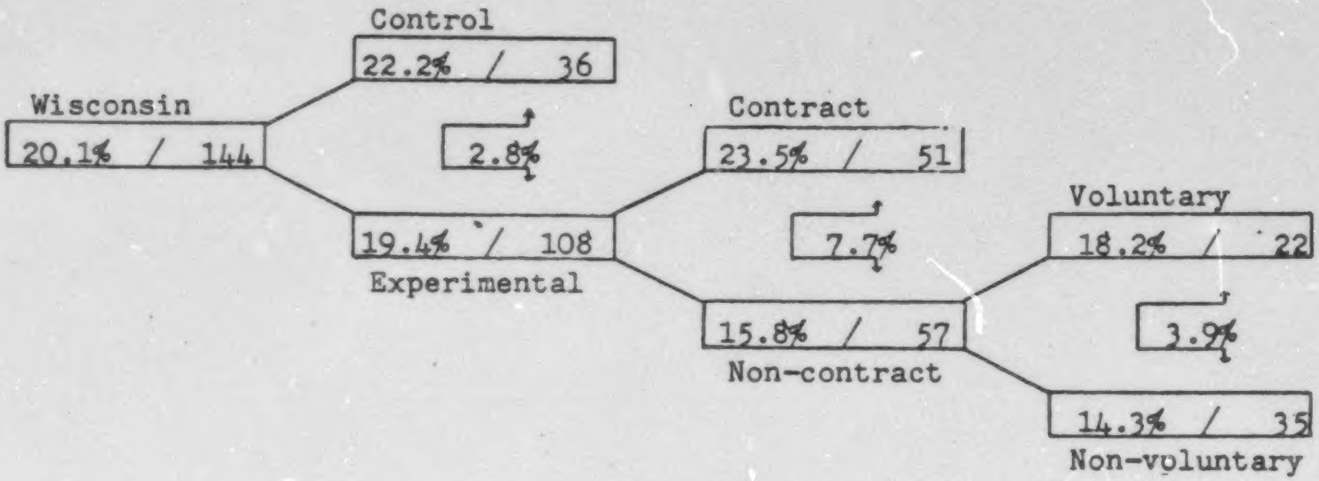

Arizona
45.8% / 166
Control
52.7% / 55
10.4%
42.3% / 111
Experimental
Contract
46.3% / 67
9.9%
36.4% / 44
Non-contract
Voluntary
38.5% / 26
5.2%
33.3% / 18
Non-voluntary

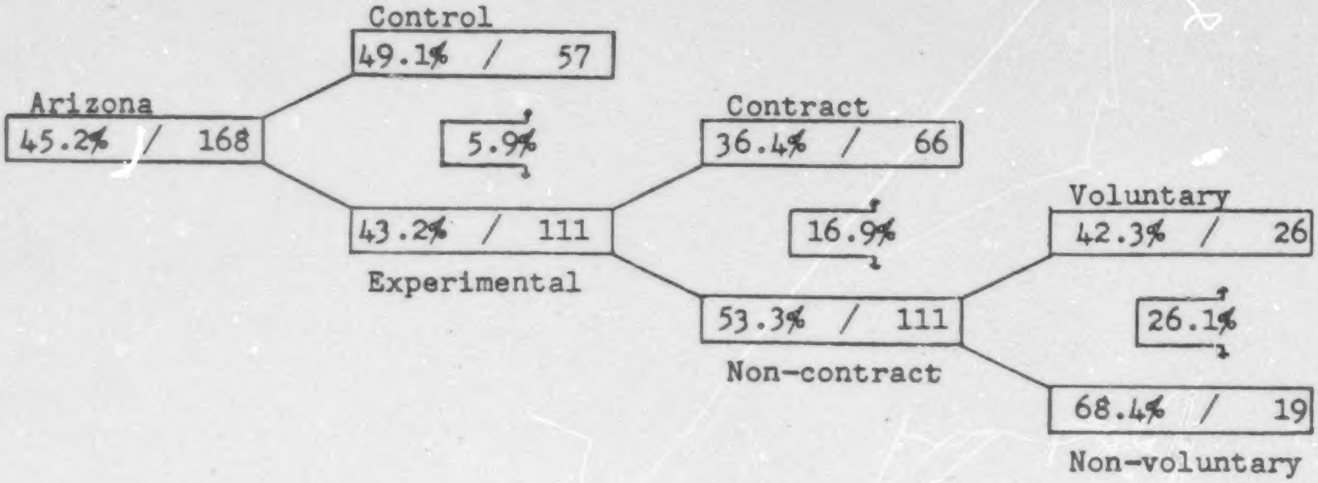
OVERALL ATTITUDE TOWARD MUTUAL AGREEMENT PROGRAMMING

WISCONSIN AND ARIZONA

At the time of intake testing, all study sample members were asked an open-ended question about their "present thoughts and feelings, questions, worries, etc. about the idea of contract programming." Narrative replies were coded according to a structure involving coder judgments about whether comments were favorable,neutral, or unfavorable toward MAP, or mixed, uncertain, or conditional in character. Nearly four-fifths of the subjects provided responses deemed codable. In Wisconsin, there appeared to be considerably greater doubt and ambivalence than in Arizona, with 20% of the former study sample, and 45% of the latter restricting themselves to only favorable narrative replies about MAP. Differences among Wisconsin comparison groups were slight; in Arizona, non-contract experimentals and, among these, those who would later be denied a contract, were substantially more likely than other samples to offer only favorable comments about MAP.

11 INTAKE
PRISONER OFFERS ONLY FAVORABLE COMMENTS ABOUT MAP IN RESPONSE TO OPEN-ENDED QUESTION ABOUT THOUGHTS, FEELINGS


Wisconsin
20.1% / 144
Control
22.2% / 36
2.8%
19.4% / 108
Experimental
Contract
23.5% / 51
7.7%
15.8% / 57
Non-contract
Voluntary
18.2% / 22
3.9%
14.3% / 35
Non-voluntary


Arizona
45.2% / 168
Control
49.1% / 57
5.9%
43.2% / 111
Experimental
Contract
36.4% / 66
16.9%
53.3% / 111
Non-contract
Voluntary
42.3% / 26
26.1%
68.4% / 19
Non-voluntary

IMPORTANCE OF KNOWLEDGE ABOUT DATE OF RELEASE

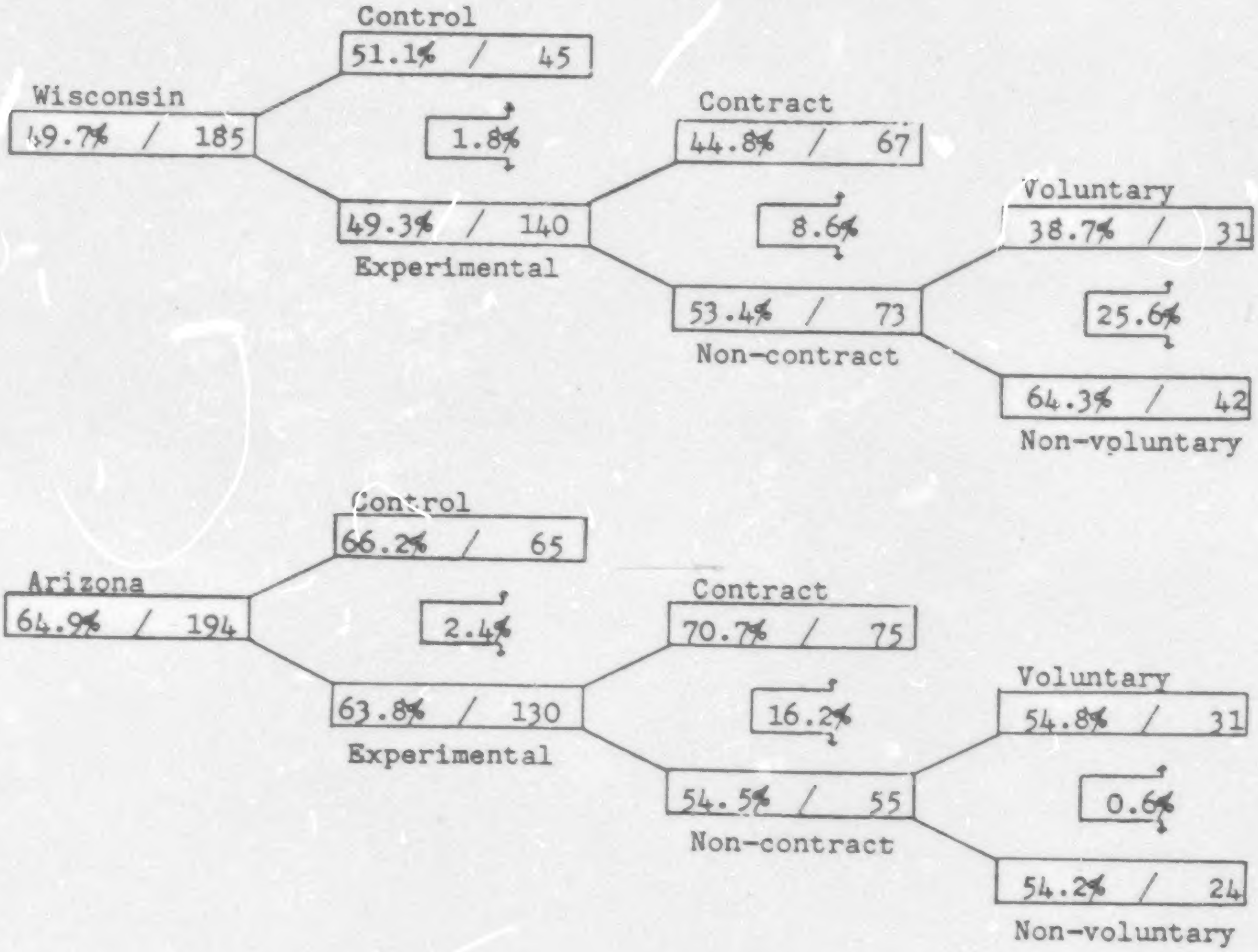
WISCONSIN AND ARIZONA

Prisoners were asked at the time of intake to how much trouble they would be willing to go in order to know just when they were released, and were supplied a set of six alternative responses ranging from "none" to "a hell of a lot." Half the Wisconsin study sample and two-thirds of the Arizona study sample chose the most extreme reply, and no difference was found in either state between experimentals and controls.

In Arizona, a difference of 16 percentage points, significant at trend level ($X^2 = 2.91$; $p < .10$) was found between contract and non-contract experimentals, with the former more anxious about obtaining certainty regarding release date, and there was no difference between voluntary and non-voluntary drops. In Wisconsin, non-voluntary and voluntary drop subsamples diverged markedly from one another (28 percentage points; $X^2 = 22.6$; $p < .01$), with the former more interested than contract cases, and the latter less interested in obtaining knowledge of release date.

12 INTAKE

PRISONER CLAIMED HE WAS WILLING TO GO TO "A HELL OF A LOT" OF TROUBLE IN ORDER TO LEARN RELEASE DATE


Wisconsin
49.7% / 185
Control
51.1% / 45
1.8%
49.3% / 140
Experimental
Contract
44.8% / 67
8.6%
53.4% / 73
Non-contract
Voluntary
38.7% / 31
25.6%
64.3% / 42
Non-voluntary
Arizona
64.9% / 194
Control
66.2% / 65
2.4%
63.8% / 130
Experimental
Contract
70.7% / 75
16.2%
54.5% / 55
Non-contract
Voluntary
54.8% / 31
0.6%
54.2% / 24
Non-voluntary

ESTIMATED TIME UNTIL PRISON RELEASE

WISCONSIN

Prisoners were asked at the time of MAP Intake testing to guess the month and year of their release from prison assuming they obtained no contract, and to make a second guess under assumption a contract were obtained. Under the no-contract assumption, about two-thirds of the Wisconsin study sample (64%) predicted their release would take place in nine or fewer months. Controls were somewhat (9 percentage points) more optimistic than experimentals on this measure, and almost identical to the contract experimental subsample. Non-contract experimentals were more pessimistic about early release (14 percentage points), and this pessimism was limited to that subsample who were subsequently to be denied contracts or non-voluntarily removed from one which was granted, suggesting some foresight on their part of difficulty with the prison administration or with behavioral problems. None of the differences among comparison groups under the no-contract assumption is statistically significant.

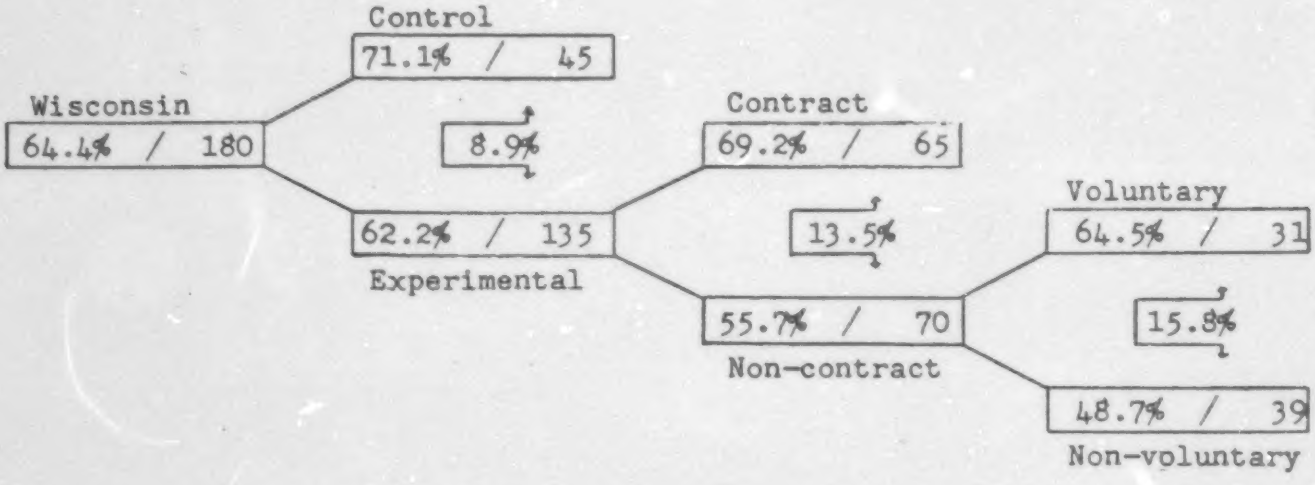
Under the second assumption--that a Mutual Agreement Program would be obtained--a substantial increase in optimism was found in the Wisconsin study sample that release would occur within nine months (88%) and this optimism was shared among all comparison groups, without substantial difference among them.

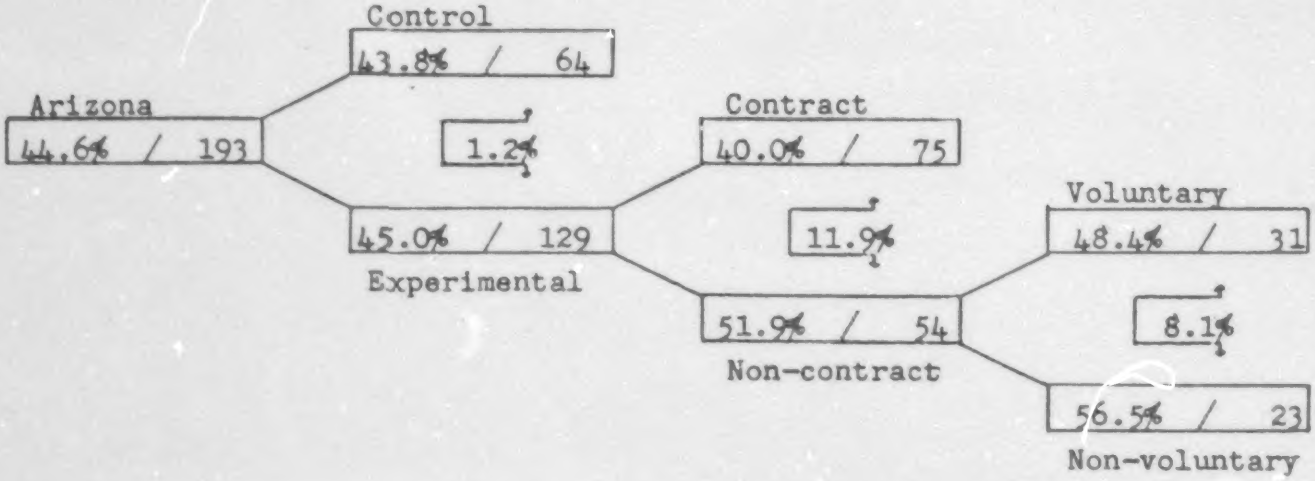
ARIZONA

Making the assumption that no contract would be secured, less than half (45%) of the Arizona study sample believed they would obtain release in nine or fewer months, and almost no difference (1 percentage point) existed between experimentals and controls on this measure. Among experimental subsamples, the direction of differences was opposite that for Wisconsin, with non-contract experimentals more optimistic than contract cases, and this optimism highest among those who were later to become non-voluntary drops.

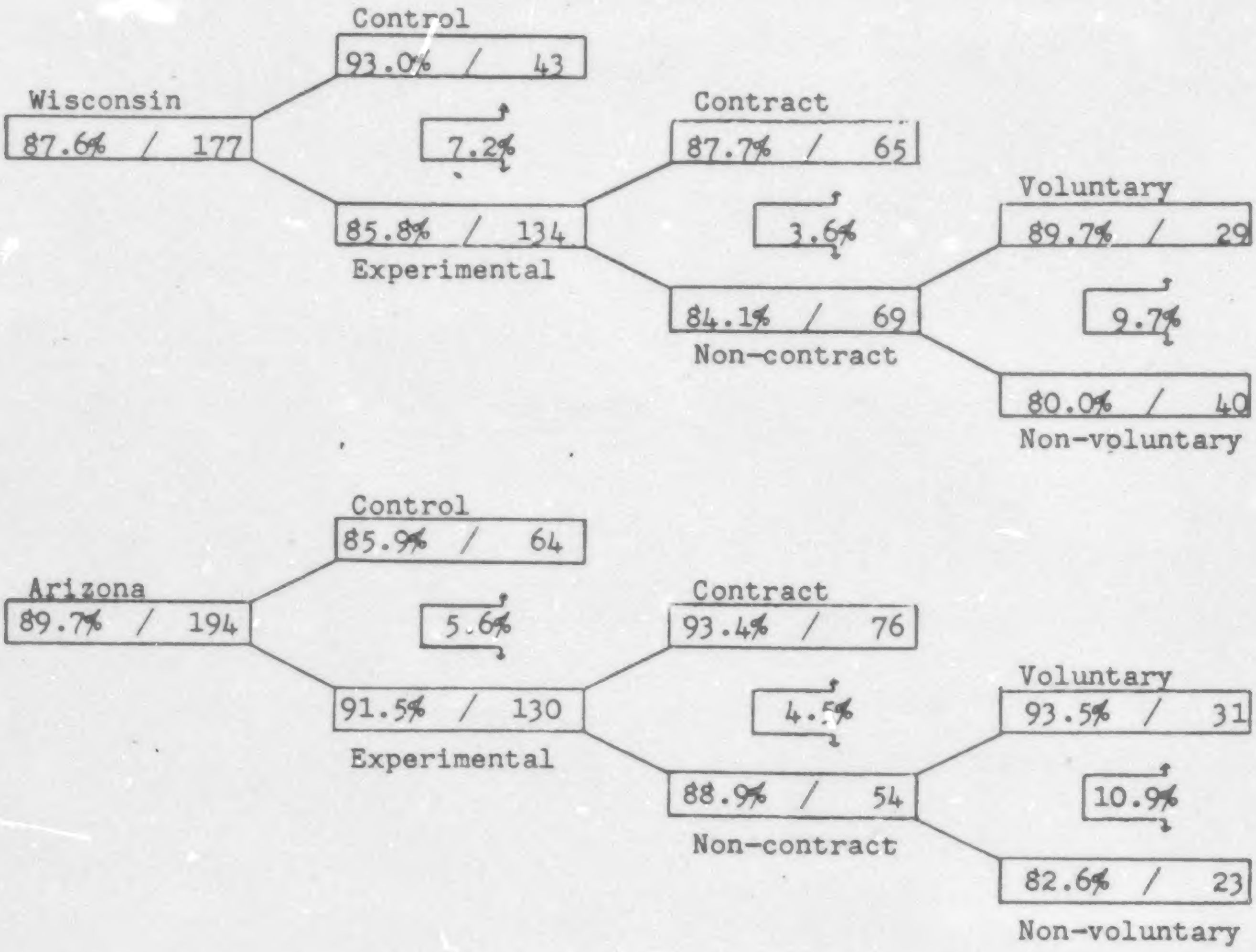
Making the assumption that they would secure a contract, optimism that prison release would be accomplished within nine months rose substantially (to 90%) in the Arizona study sample, with no significant differences present between comparison groups, but a shift in pattern occuring with the non-voluntary drops becoming relatively least optimistic, whereas under the no-contract assumption they had been relatively most optimistic.

13 INTAKE
PRISONER ESTIMATED TIME REMAINING BEFORE RELEASE IF NO CONTRACT OBTAINED WAS 9 MONTHS OR LESS


Control
71.1% / 45
Wisconsin
64.4% / 180
8.9%
Contract
69.2% / 65
62.2% / 135
Experimental
13.5%
Voluntary
64.5% / 31
55.7% / 70
Non-contract
15.8%
48.7% / 39
Non-voluntary


Control
43.8% / 64
Arizona
44.6% / 193
1.2%
Contract
40.0% / 75
45.0% / 129
Experimental
11.9%
Voluntary
48.4% / 31
51.9% / 54
Non-contract
8.1%
56.5% / 23
Non-voluntary

14 INTAKE
PRISONER ESTIMATED TIME REMAINING BEFORE RELEASE IF A CONTRACT OBTAINED WAS 9 MONTHS OR LESS


Control
93.0% / 43
Wisconsin
87.6% / 177
7.2%
Contract
87.7% / 65
Voluntary
85.8% / 134
Experimental
3.6%
89.7% / 29
84.1% / 69
9.7%
Non-contract
80.0% / 40
Non-voluntary
Control
85.9% / 64
Arizona
89.7% / 194
5.6%
Contract
93.4% / 76
91.5% / 130
Experimental
4.5%
Voluntary
93.5% / 31
88.9% / 54
10.9%
Non-contract
82.6% / 23
Non-voluntary

COMPARISONS AT INTAKE

SUMMARY

EXPERIMENTAL VS. CONTROL

BACKGROUND VARIABLES

Eight background variables--commitment offense, prior imprisonment, time served, age, ethnic group, education, and prior hourly and monthly earnings--were examined in two states--Wisconsin and Arizona--as a check upon the success of randomization in establishing equivalent comparison groups. Chi square tests for significance of difference between experimental and control samples were employed, using two-tailed tests and dichotomized variables (except for commitment offense, which was instead collapsed to three categories), with dichotomies ordinarily determined by a median split of the frequency distribution. Given sixteen tests (8 in each state), chance alone could be expected to yield one difference reaching trend significance level ($p < .10$). One difference was found, in Arizona which could be expected to have been produced, by chance, less often than once in forty times ($p < .025$), indicating that a smaller proportion of controls than experimentals--about one-fifth fewer--had completed the median period for all Arizona subjects of fifteen months in prison on their current stay. Given that other marked differences were not found between the two samples, the result for time served suggests the possibility that experimentals tended to be further into their terms, or nearer release date than might be expected for controls. If this was the case, however, there is no evidence to be found from the subjects themselves--experimentals and controls were equally likely to guess that, without a MAP agreement, they would attain release within nine months of project intake. Another possibility is that the experimentals were not, in fact, "nearer release" at time of intake, but that the difference reflects their tendency (statistically non-significant) to have, more often than controls, been committed for offenses involving threat or violence.

A search was conducted to see if the difference could be traced to a flaw in the randomization procedure. Randomization had been carried out by use of a rotating drum into which cards representing the subjects were placed, mixed, and then withdrawn by prisoners, and had taken place on six separate occasions with the first cards drawn designating controls. The most likely factor was an original order associated with time served--prison serial number was suspect--and inadequate mixing during rotation of the drum. The state project coordinator has reported that, to the extent there was an orderly initial arrangement, it was determined by project identification number rather than prison serial number--a check, using the Wald-Wolfowritz runs test confirms inadequate mixing relative to project ID number--sequences of consecutive numbers within experimental and control samples show fewer runs than would be expected to occur by chance. However, a check for connection between project ID number and prison serial number reveals insufficient association to account for the problem. Several other techniques have been attempted to determine how the time-served difference could have arisen from procedural defect, but none of these succeeds in providing any explanation better than "fate."

Apart from the above problem, the basic comparison groups in Wisconsin and Arizona appear on other initial measures to meet acceptable standards of equivalence, permitting the inference that differences found on measures taken later in time may be attributed to consequences of differential program effects. To the extent that slight differences across measures yield any advantage, the "edge" in Wisconsin goes to experimentals, with more low-recidivism type offenses, more first termers, and more past earners of $3.00/hr. or more. Similarly, in Arizona, more low-recidivism offenses, fewer ethnic minority members, and more cases with high school educations are found among experimentals, though controls held an edge in past monthly earnings. Although sufficient initial equivalence seems to exist between the full experimental and the control samples to permit straightforward interpretation of subsequent performance differences as program effects, selection and self-selection effects operative at the initial stages rule out the legitimacy--particularly, in Arizona--of performance comparisons between controls and the contract experimental subsamples. These early effects introduced a significant ($p < .05$) difference on commitment offense in Arizona, and trend differences ($p < .10$) on prior record in Wisconsin and ethnic status in Arizona. Other differences, though of lesser magnitude, were in a direction suggesting that contract experimentals, regardless of any program effect, might be expected to perform more successfully after release.

BELIEF AND OPINION MEASURES

On items at intake dealing with overall attitude toward Mutual Agreement Programming, personal investment in obtaining knowledge of prison release date, and estimated time remaining in prison if no Mutual Agreement Programming contract were secured or it one were secured, no statistically significant differences existed between experimentals and controls in either Wisconsin or Arizona, and percentage differences were slight except for somewhat greater optimism about early release present among Wisconsin controls. Between states, Arizona prisoners appeared to feel greater urgency for knowledge of release date, to be more pessimistic about early release without MAP, and to view the program more favorably than prisoners in Wisconsin.

CONTRACT VS. NON-CONTRACT

BACKGROUND VARIABLES

As with the experimental-control comparisons, 16 tests on background measures would yield by chance, roughly one difference significant at $p < .10$ if no selection effects were operative in determining which experimental cases secured and remained under contract. One such difference does arise, in Wisconsin, on the variable of prior imprisonment, with contract cases less likely than those without contract to have such a record. This difference appears to be almost entirely attributable to greater propensity among first termers to decline contract entry voluntarily--a self-selection effect. (Also in Wisconsin, a difference between voluntary and non-voluntary drops from contract was found significant at $p < .01$ on education completed, and the pattern of difference suggests an administrative screening effect--that subjects with high school education completed found it harder to be accepted into contract.) In Arizona, a difference significant at $p < .025$ reveals cases securing a contract were more likely to have homicide, assault, or robbery commitment offenses, and less likely to have sex or drug offenses, with the difference present as compared to both voluntary and non-voluntary cases without contract, but more pronounced for the latter; the pattern suggests some self-selection but also, and more powerfully, an administrative screening effect. The latter might arise either from belief in greater risk and need or belief in greater ability to benefit.

No other differences between contract and non-contract cases on background variables were statistically significant; but nevertheless, the overall pattern of lesser differences makes it difficult to sustain the assumption that selection effects are self-cancelling, and that contract and non-contract subsamples may be treated as equivalent for purposes of subsequent performance comparisons.

BELIEF AND OPINION MEASURES

Only one difference reaching trend significance threshold ($p < .10$) was found between contract and non-contract cases, and that occurred in Arizona, where contract cases were more likely to claim willingness to go to "a hell of a lot" of trouble to learn release date. Surprisingly, no difference at all existed between voluntary and non-voluntary Arizona drops from contract on this measure. Such a difference was, however, found in Wisconsin, significant at $p < .01$, with voluntary drops showing less investment than contract cases, non-voluntary drops more investment, and the opposing directions cancelling one another on the contract vs. non-contract comparison.

Since all comparison groups in the study sample evidenced considerable faith in MAP having some effect to accelerate release, and since one would reasonably assume that a major disincentive to involvement in contract programming would be optimism about early release regardless of involvement, it is surprising that prisoners who declined contract programming showed no substantially greater likelihood than other groups to predict their own release in nine months when assuming no contract secured, and no lesser likelihood when assuming a contract secured.

STATUS AT THE POINT OF PRISON RELEASE

AND

DIFFERENCES AMONG COMPARISON GROUPS

WISCONSIN

Two-thirds (64%) of the Wisconsin study sample had anticipated prison release within nine months of becoming MAP eligible, regardless of whether a contract was obtained. These anticipations proved unduly optimistic, in that only two-fifths (44%) actually achieved release from prison in that period of time. Experimentals had a slight (7 percentage points) but statistically non-significant edge over controls in attaining release within nine months from the point of initial inquiry. Contract experimentals enjoyed a lead of 22 percentage points over experimentals without contract--a difference statistically significant at $p < .025$ ($X^2 = 6.39$). Even so, about 12% fewer of the experimentals with contract obtained release than had expected to do so even if they obtained no contract, and 30% fewer than expected if a contract were obtained. The difference between contract and non-contract experimentals is primarily attributable to the especially low rate of release among the non-voluntary subsample. Within this subsample, there was no difference between those denied contract entry, and those who suffered cancellation of a contractual agreement--22% of the former and 24% of the latter attained release in nine months. The percent released among the voluntary subsample of experimentals who declined contract entry or withdrew from programming was almost as high as that for experimentals who completed contracts, and significantly ($X^2 = 5.49$; $p < .025$) higher than for the non-voluntary subsample.

All Wisconsin contract cases were released by the end of August, 1973. A check at that time revealed that 58% of controls, and still only 35% of non-contract experimentals had also been released; the percent released among the full experimental sample was 65%, and the difference--seven percent greater than controls, was unchanged from the earlier check and still attributable to chance.

A final check at the close of the data collection period revealed that 88% of Wisconsin controls and 87% of Wisconsin experimentals had attained release from prison by the end of June, 1974. Despite the fact that 100% of contract experimentals had achieved release ten months before this check, the experimental-control difference was fully eroded away in subsequent months by retardation in the release rate of non-contract experimentals. The difference between the voluntary and non-voluntary subsamples remained significant ($X^2 = 6.05$; $p < .025$), with the former about identical to controls (90% released), and the latter about 25 percentage points lower (64% released).

In a search for possible net effects on time served, mean periods of incarceration were calculated for experimentals and controls. MAP experimentals had served an average of 13.0 months prior to MAP intake testing, and MAP controls had served 13.3 months--a mean difference of about nine days. By the close of the data collection period, MAP experimentals who had been released were found to have served an average of 22.9 months, compared to 23.8 months for released controls, and inclusion of subjects not yet released yielded months-served-to-date figures of 24.2 months for the full experimental sample, vs. 25.3 months for the full control sample. It is problematic whether this slight difference will grow larger or vanish entirely when those subjects still in prison eventually attain release.

ARIZONA

In Arizona, 45% of the members of the study sample had anticipated release in nine months regardless of MAP, and 90% expected release by that time if they managed to get and retain a contract. Sixty-four percent of the full study sample actually achieved release within the nine months period, and the prisoner predictions turned out, in some ways, to be remarkably accurate: If we compare the percent of released controls (42%) against the control sample prediction under a no-contract assumption, the estimated vs. actual difference is only 2 percentage points; if we compare the percent of released contract experimentals (95%) against prediction by that subsample under the contract-obtained assumption, the estimated vs. actual difference is again only 2 percentage points.

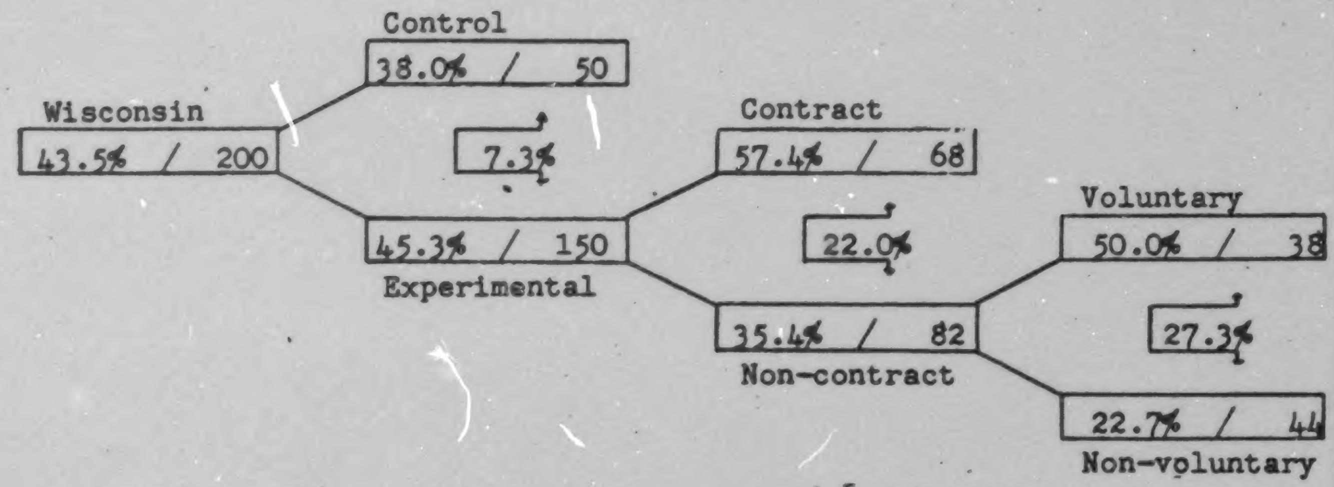
Seventy-five percent of the full Arizona experimental sample were released by the nine-month checkpoint, vs. 42% of controls, a difference which is statistically highly significant ($X^2 = 20.13; p < .001$). Even the non-contract experimental sample, in total, had achieved a slightly higher proportion of releases than the controls, and was doing nearly as well as its members had predicted under a no-contract assumption (48% actual release, vs. 52% predicted). Individual predictions were nevertheless quite inaccurate among the non-contract members, as is evident upon examination of the voluntary and non-voluntary subsamples, with the former group (which consisted almost entirely of decliners, rather than withdrawers) netting much better than anticipated (74% released vs. 48% predicted) and the latter (which consisted almost entirely of denieds rather than removeds) much poorer (13% released, vs. 56 % predicted).

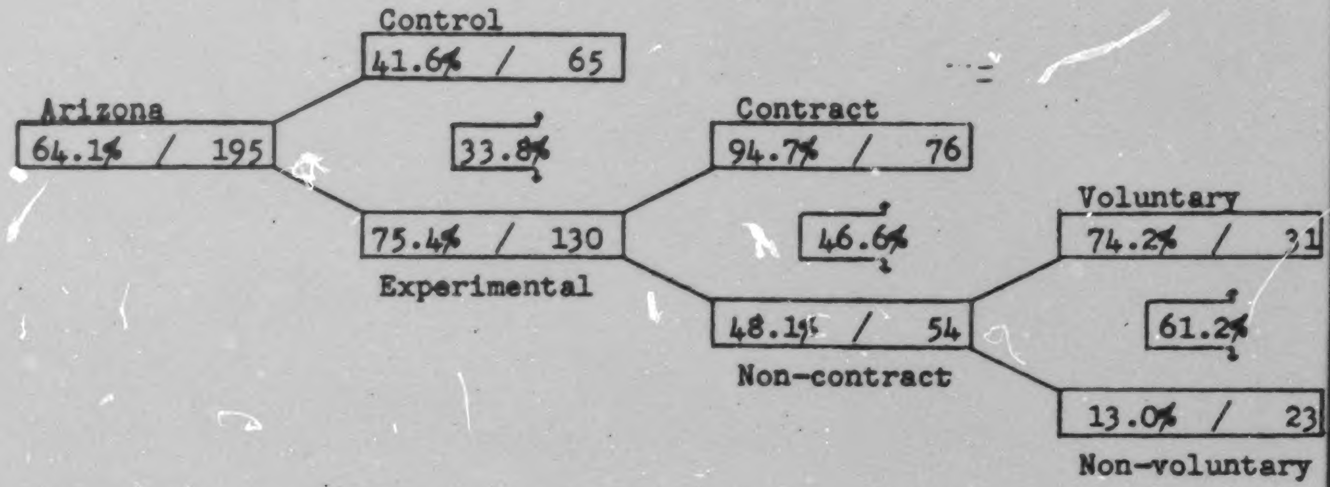
The differences in actual release proportions between the experimental subsamples are statistically highly significant (contract vs. non-contract: $X^2 = 17.4; p < .001$).

The final check at close of data collection revealed that all the differences established among comparison groups at the earlier check continued to be manifest through the end of May, 1974, although the magnitude of such differences naturally grew less with the passage of time and attainment of high release proportions among most subsamples. Eighty-two percent of the full Arizona study sample was released by the cut-off date. Experimentals still held a 16 percentage point edge over controls ($X^2 = 6.48; p < .025$), contract experimentals a 28 percentage point edge over those without contracts ($X^2 = 19.84; p < .001$), and voluntary drops a 39 percentage point edge over non-voluntary drops ($X^2 = 7.97; p < .005$). Essentially identical proportions of the control and the non-contract experimental samples had been released.

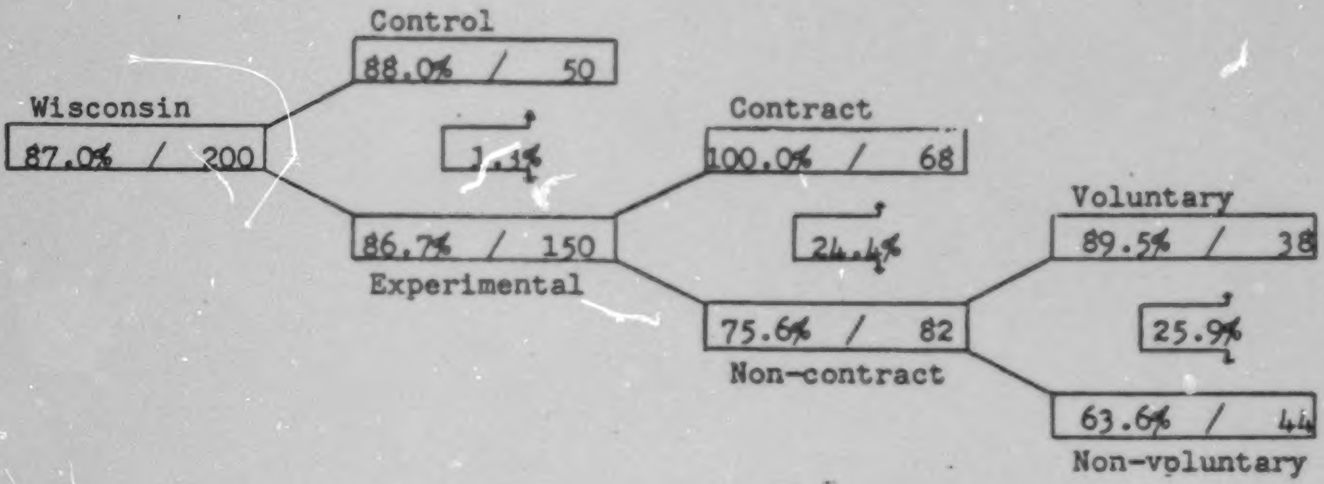
The pattern of findings just presented for Arizona would appear to augur well for the demonstration of a dramatic saving in prison time served attributable to the MAP project in that state. Upon comparison of total time served, however, the earlier reported finding of an initial difference between experimental and control samples in the amount of time served prior to project entry spoils the demonstration of any such effect. Comparison of median time served from prison admission to data collection cut-off date yields identical medians of 24 months for both the experimental and control samples. The finding of no difference upon resort to the median becomes a finding of negative difference--more time in prison for experimentals than for controls--when the mean is employed; this result is attributable to the fact that the time served distribution is positively (and markedly) skewed. Using the mean, we find that experimentals had served an average of 33.6 months between prison admission and 5/31/74 cut-off date, compared to 30.2 months for controls. If comparison is limited to only those subjects who had attained release, the averages are 33.4 months for experimentals, and 29.3 months for controls. By either form of comparison, controls show about three months less total time served than experimentals, despite the fact that, if only the portion of incarceration following entry to the MAP study sample is examined, controls averaged three months more than experimentals.

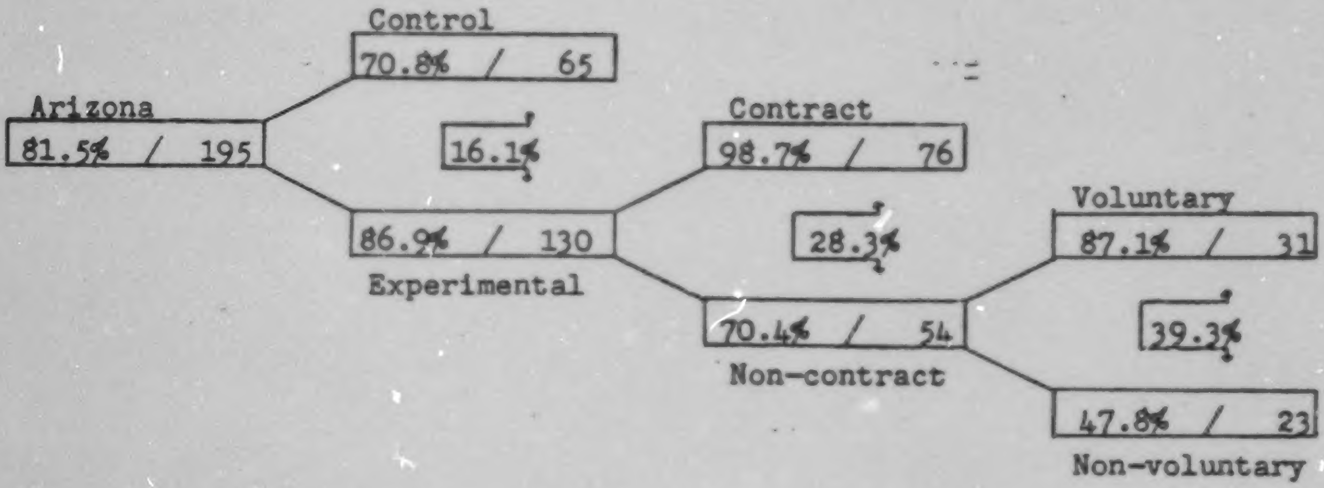
18 RELEASE
ACTUAL TIME FROM STUDY SAMPLE ENTRY TO RELEASE WAS 9 MONTHS OR LESS


Control
38.0% / 50
Wisconsin
43.5% / 200
7.3%
Contract
57.4% / 68
45.3% / 150
Experimental
22.0%
Voluntary
50.0% / 38
35.4% / 82
Non-contract
27.3%
22.7% / 44
Non-voluntary


Control
41.6% / 65
Arizona
64.1% / 195
33.8%
Contract
94.7% / 76
75.4% / 130
Experimental
46.6%
Voluntary
74.2% / 31
48.1% / 54
Non-contract
61.2%
13.0% / 23
Non-voluntary

16 RELEASE
ACTUAL RELEASE BY OR PRIOR TO END OF JUNE, 1974 (WISCONSIN) OR END OF MAY, 1974 (ARIZONA)


Wisconsin
87.0% / 200
Control
88.0% / 50
1.3%
Experimental
86.7% / 150
Contract
100.0% / 68
24.4%
Non-contract
75.6% / 82
Voluntary
89.5% / 38
25.9%
Non-voluntary
63.6% / 44


Arizona
81.5% / 195
Control
70.8% / 65
16.1%
Experimental
86.9% / 130
Contract
98.7% / 76
28.3%
Non-contract
70.4% / 54
Voluntary
87.1% / 31
39.3%
Non-voluntary
47.8% / 23

ATTRIBUTION OF CRIMINALITY TO ECONOMIC CAUSES

WISCONSIN

Prisoners were asked the following question as the time of their release approached:

"How much do you think low income or lack of work had to do with causing the offense that brought you to prison?"

In Wisconsin, the distribution of replies was:

NONE OF THE CAUSE	33.7%
A LITTLE	12.7
A FAIR AMOUNT	24.3
MOST OF THE CAUSE	29.3

A majority (53.6%) agreed that economic factors were at least "a fair amount" of the forces leading to their commitment offense, and a negligible (1 percentage point) difference existed between experimentals and controls. Among the experimental subsamples, a slight (4 percentage point) difference existed between the voluntary and non-voluntary drops, but a substantial (32 percentage point) difference was found between non-contract experimentals and those completing contracts, with the latter far more likely to see low income or unemployment as responsible for their offense. The difference is statistically significant ($X^2 = 9.55$; $p < .005$), but its origin is ambiguous: since no equivalent item had been included at the point of intake testing, it is not possible to determine whether a selection effect (i.e., those with greater apparent and self-recognized need for training being more interested in and accepted for MAP programming), or an indoctrination effect (i.e., those under contract coming to echo the program ideology), or a combination of both is necessary to explain the phenomenon. However, since non-contract experimentals--even the voluntary ones--are considerably lower than controls on this measure, the selection hypothesis seems most tenable, especially given that the overall experimental-control difference is slight.

ARIZONA

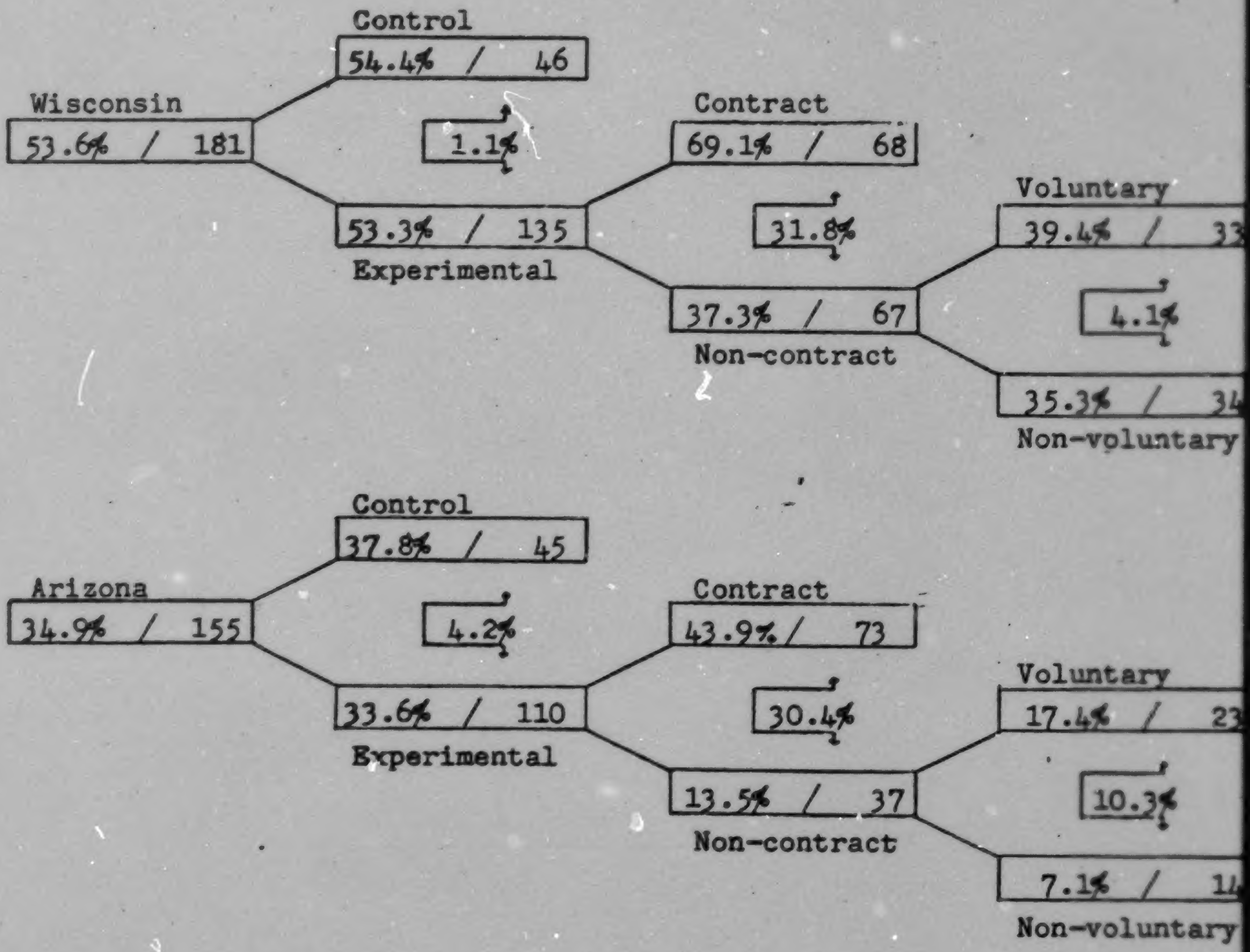
Arizona prisoners in the MAP study sample were less inclined to attribute their offense to economic causes:

NONE OF THE CAUSE	52.9%
A LITTLE	12.3
A FAIR AMOUNT	15.5
MOST OF THE CAUSE	19.4

Though only a minority (35%) ascribed at least "a fair amount" of responsibility for their crime to low income or lack of work, and the difference between experimentals and controls was slight (4 percentage points), contract experimentals were, as in Wisconsin, dramatically more likely (30 percentage points) than non-contract experimentals to interpret criminality by resort to economic causality; the difference is, again, statistically significant ($X^2 = 8.$[illegible]; $p < .005$).

17 RELEASE

PRISONER BELIEVED LOW INCOME OR LACK OF WORK AT LEAST "A FAIR AMOUNT" OF CAUSE FOR HIS LAST OFFENSE


Control
54.4% / 46
Wisconsin
53.6% / 181
1.1%
Contract
69.1% / 68
Voluntary
39.4% / 3
53.3% / 135
Experimental
31.8%
37.3% / 67
Non-contract
4.1%
35.3% / 3
Non-voluntary
Control
37.8% / 45
Arizona
34.9% / 155
4.2%
Contract
43.9% / 73
Voluntary
17.4% / 2
33.6% / 110
Experimental
30.4%
13.5% / 37
Non-contract
10.3%
7.1% / 1
Non-voluntary

BELIEF IN PRISON PROGRAMS ENHANCEMENT OF JOB OPPORTUNITY

WISCONSIN

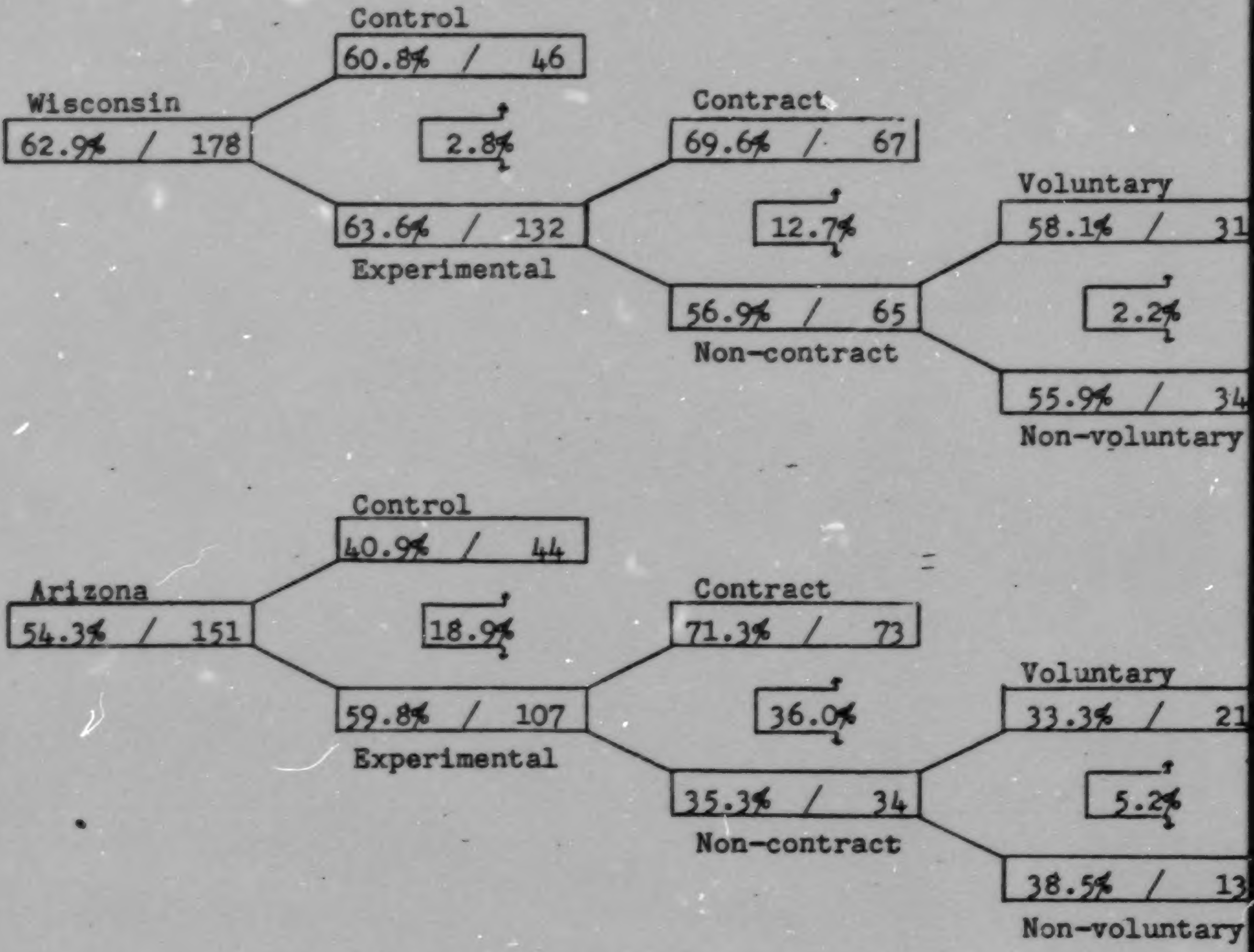
Sixty-three percent of prisoners in the Wisconsin study sample credited prison programs with assisting them at least "a fair amount" to obtain a decent job when released. There was essentially no overall difference (3 percentage points) between experimentals and controls on this measure although, within the experimental sample, contract cases were more likely (13 percentage points) than non-contract cases to indicate this kind of appreciation as their release approached. Between the voluntary and non-voluntary subsamples of experimentals without contract, there was no difference.

ARIZONA

Fifty-four percent of prisoners in the Arizona study sample believed prison programs had enhanced their job opportunity by at least a fair amount, and a moderate (19 percentage point) difference was found between experimentals and controls, with the former group showing greater faith in the effects of programming. While this difference reached only a trend level of statistical significance ($X^2 = 3.76$; $p < .10$), it should also be noted that the large (36 percentage point) and statistically significant difference ($X^2 = 11.01$; $p < .005$) between contract and non-contract experimentals in Arizona was achieved primarily by a substantial elevation in the contract subsample, rather than by decrement in the non-contract subsample.

18 RELEASE

PRISONER BELIEVED PRISON PROGRAMS HELPED HIM AT LEAST "A FAIR AMOUNT" TOWARD OBTAINING A DECENT JOB


Wisconsin
62.9% / 178
Control
60.8% / 46
2.8%
63.6% / 132
Experimental
Contract
69.6% / 67
12.7%
56.9% / 65
Non-contract
Voluntary
58.1% / 31
2.2%
55.9% / 34
Non-voluntary
Arizona
54.3% / 151
Control
40.9% / 44
18.9%
59.8% / 107
Experimental
Contract
71.3% / 73
36.0%
35.3% / 34
Non-contract
Voluntary
33.3% / 21
5.2%
38.5% / 13
Non-voluntary

BELIEF IN MAP'S ASSISTANCE FOR RELEASE DATE CERTAINTY

WISCONSIN AND ARIZONA

At the time of release, prisoners were asked a series of ten items about various ways in which MAP might have helped them, with those under contract asked to respond in terms of whether it had helped them, and those not under contract to indicate their belief about whether a contract would have helped.

The item among the ten which received heaviest endorsement in both states that MAP had, or would have helped "a lot"* was its effect on "making my release date more certain." Seventy-two percent of Wisconsin prisoners and 71% of Arizona prisoners gave MAP this credit. This belief was shared equally by experimentals and controls in Arizona, but controls in Wisconsin were significantly more skeptical than experimentals ($X^2 = 9.90$; $p < .005$) that a contract would have helped them in this way. In both Wisconsin and Arizona, contract experimentals were substantially more likely to believe MAP had helped a lot on certainty of release than for non-contract experimentals to believe it would have helped them. No difference existed between voluntary and non-voluntary drops in Arizona, but in Wisconsin, a significant difference ($X^2 = 4.35$; $p < .05$) was found--a difference attributable to greater skepticism on the part of those who declined or withdrew from a MAP agreement; prisoners denied a contract, and those removed from programming were equally likely to show faith in MAP on this measure, and as likely to do so as those who completed contracts.

For the entire two-state study sample, only 9% of prisoners expressed a belief that MAP was "no help" relative to certainty of release date. (Seventy-one percent of the total had said it was "a lot" of help.)

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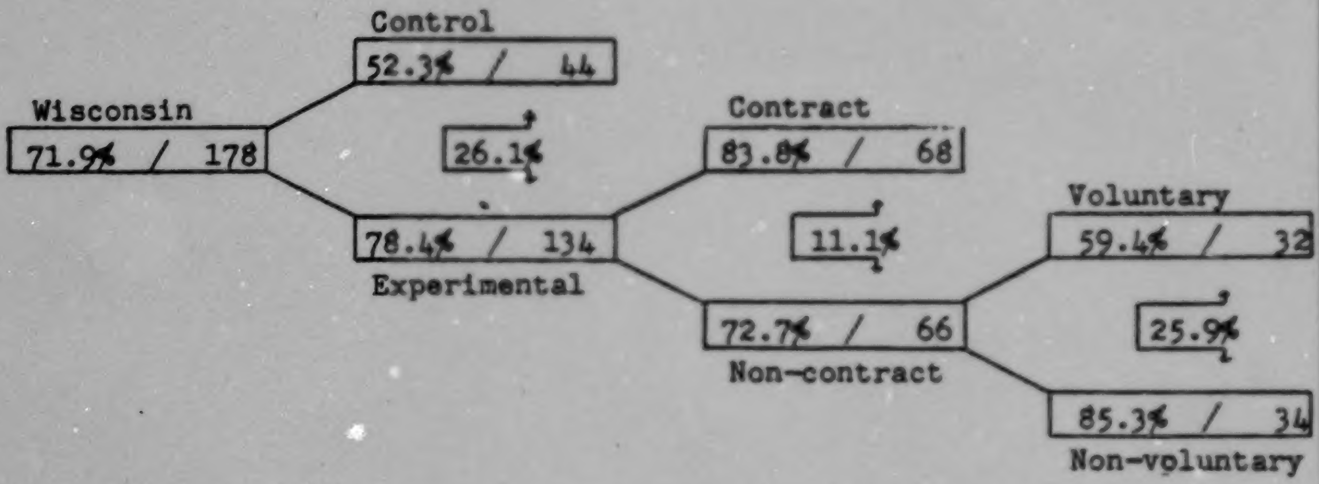
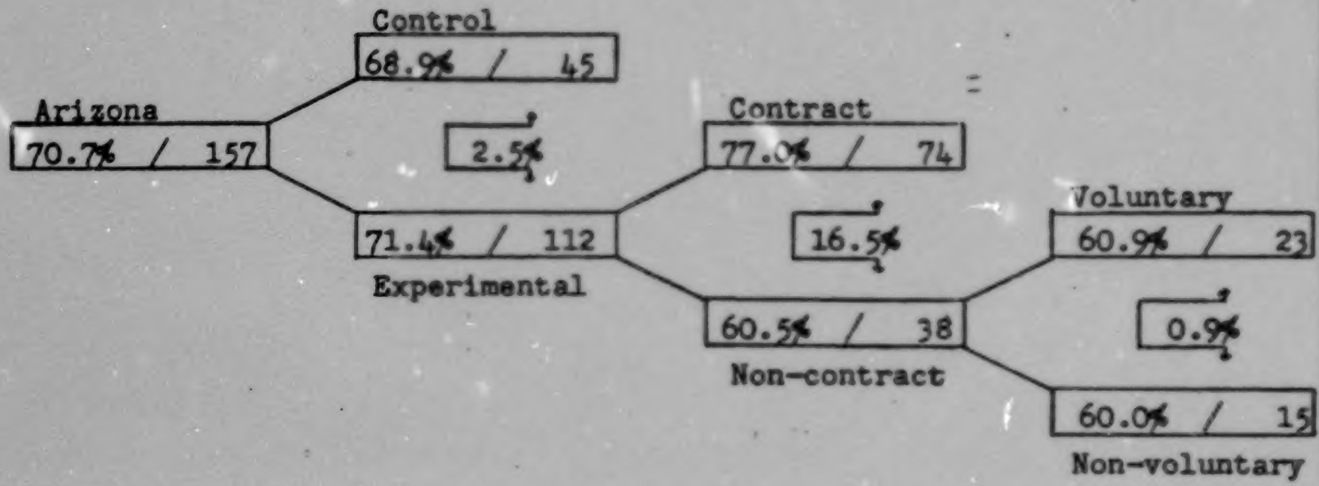
*Four response alternatives were offered--"a lot," "a fair amount," "a little," and "none." Nearly half (44%) the responses to the full ten-item set were "a lot," and that alternative is used throughout this section of the report to provide a constant threshold for inter-item comparison.

19 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN OBTAINING A MORE CERTAIN RELEASE DATE


Wisconsin
71.9% / 178
Control
52.3% / 44
26.1%
78.4% / 134
Experimental
Contract
83.8% / 68
11.1%
72.7% / 66
Non-contract
Voluntary
59.4% / 32
25.9%
85.3% / 34
Non-voluntary


Arizona
70.7% / 157
Control
68.9% / 45
2.5%
71.4% / 112
Experimental
Contract
77.0% / 74
16.5%
60.5% / 38
Non-contract
Voluntary
60.9% / 23
0.9%
60.0% / 15
Non-voluntary

BELIEF IN MAP'S ASSISTANCE FOR OUTSIDE PLANS

WISCONSIN AND ARIZONA

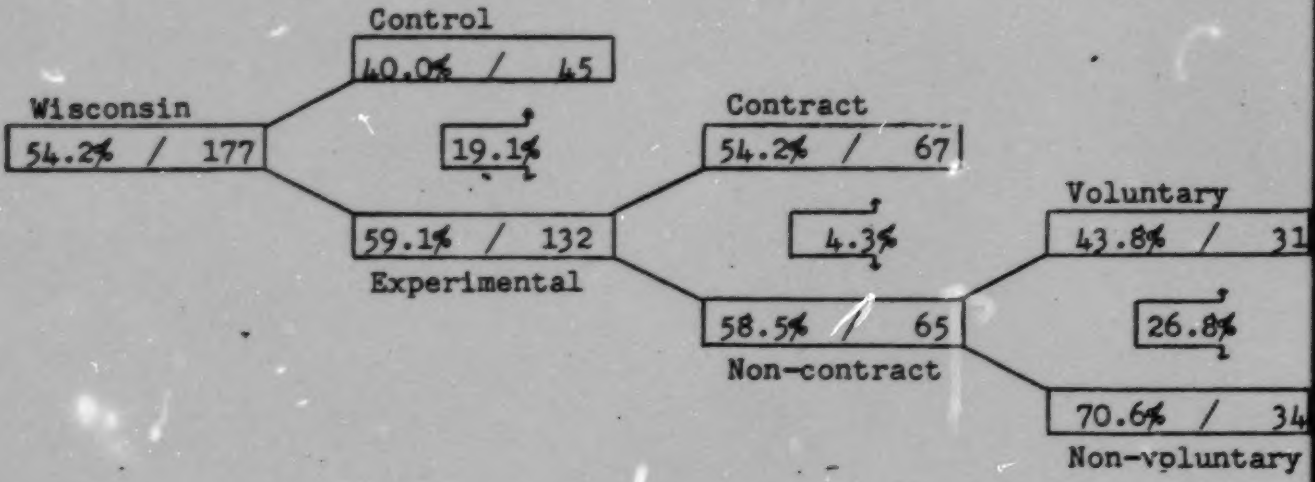
Of the ten areas in which MAP might be credited, "helping me plan and make arrangements outside because I knew when I'd be going home" was second on level of endorsement in Wisconsin, where 54% agreed it would or did help "a lot," and third in Arizona, where 68% agreed.

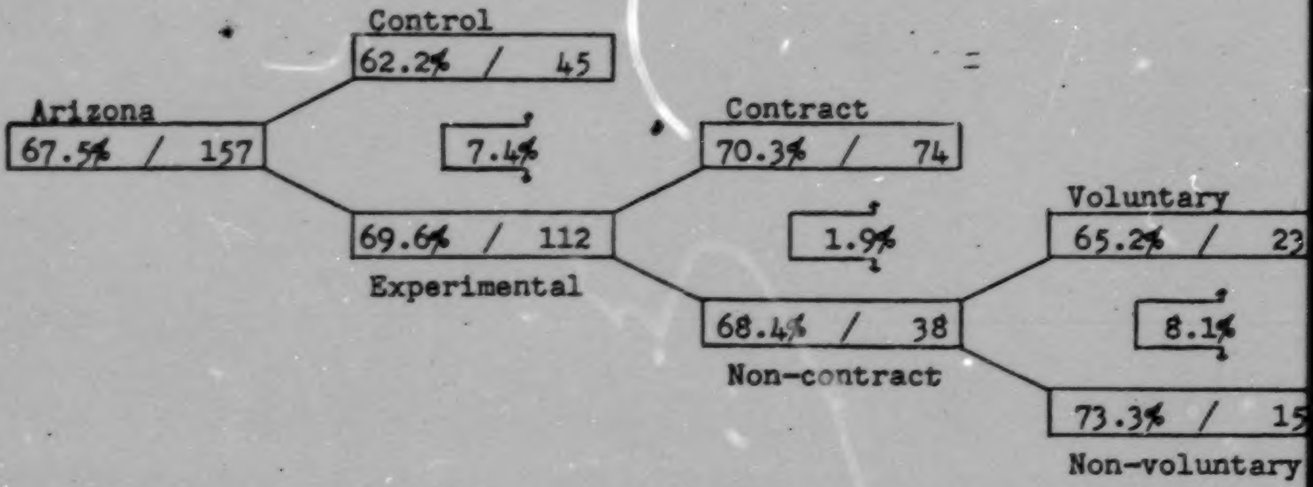
Differences among the various comparison groups in Arizona were slight. In Wisconsin, controls were again significantly more skeptical ($X^2 = 4.19$; $p < .05$) than experimentals, and voluntary drops from contract tended, at trend significance level ($X^2 = 3.33$; $p < .10$), to be more skeptical than non-voluntary drops. Within the non-voluntary subsample, 79% of those denied a contract agreed it would have helped a lot in planning, compared to 60% of those who obtained but then lost a contract.

For the combined two-state study sample, only 11% expressed the opinion that MAP would have been "no help" relative to outside plans. "Sixty percent of the total said it was "a lot" of help.

20 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN MAKING PLANS OUTSIDE


Wisconsin
54.2% / 177
Control
40.0% / 45
19.1%
59.1% / 132
Experimental
Contract
54.2% / 67
4.3%
58.5% / 65
Non-contract
Voluntary
43.8% / 31
26.8%
70.6% / 34
Non-voluntary


Arizona
67.5% / 157
Control
62.2% / 45
7.4%
69.6% / 112
Experimental
Contract
70.3% / 74
1.9%
68.4% / 38
Non-contract
Voluntary
65.2% / 23
8.1%
73.3% / 15
Non-voluntary

BELIEF IN MAP'S EFFICACY FOR REDUCING LENGTH OF PRISON STAY

WISCONSIN AND ARIZONA

"Getting me out of prison earlier" placed second out of ten on items of endorsement in Arizona, and third in Wisconsin. Forty-three percent of Wisconsin prisoners indicated that it either did or would have made "a lot" of difference. A 13 percentage point difference existed between experimentals and controls, and this difference is largely attributable to greater belief among non-contract experimentals that MAP would have made a difference in their case, rather than belief among contract experimentals that it had actually made a difference for them (difference = 17 percentage points, $X^2 = 3.04$; $p < .10$).

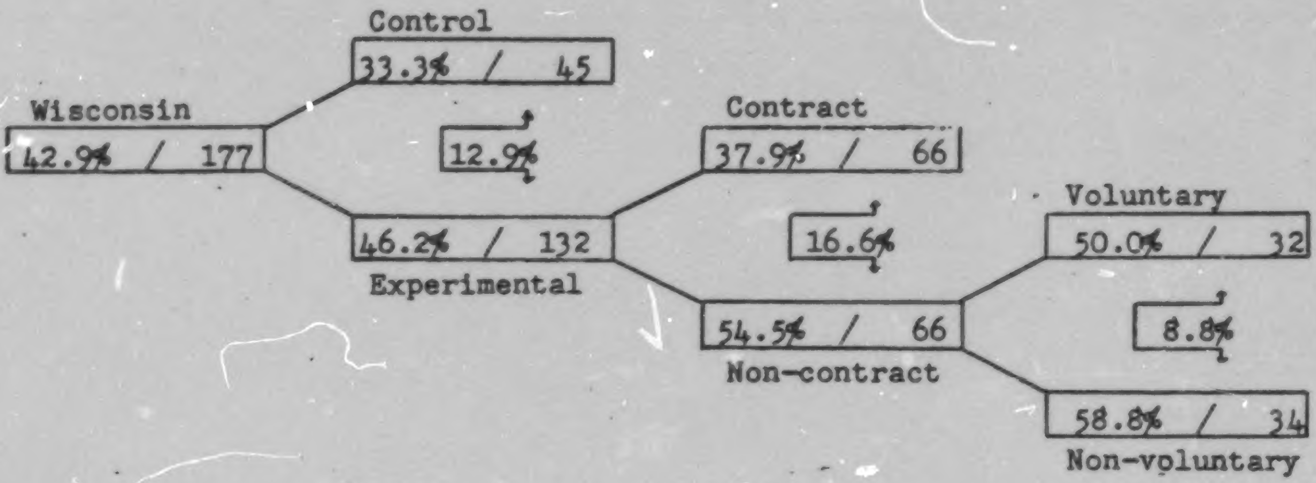
Members of the Arizona study sample were substantially more likely than Wisconsin subjects to perceive MAP as abbreviating the period of incarceration, with 70% responding that an agreement made or would have made a lot of difference. Further, in Arizona, the level of such belief was uniformly held over all comparison groups, with differences of only one or two percent between experimentals and controls, contract and non-contract experimentals, and voluntary and non-voluntary contract drops.

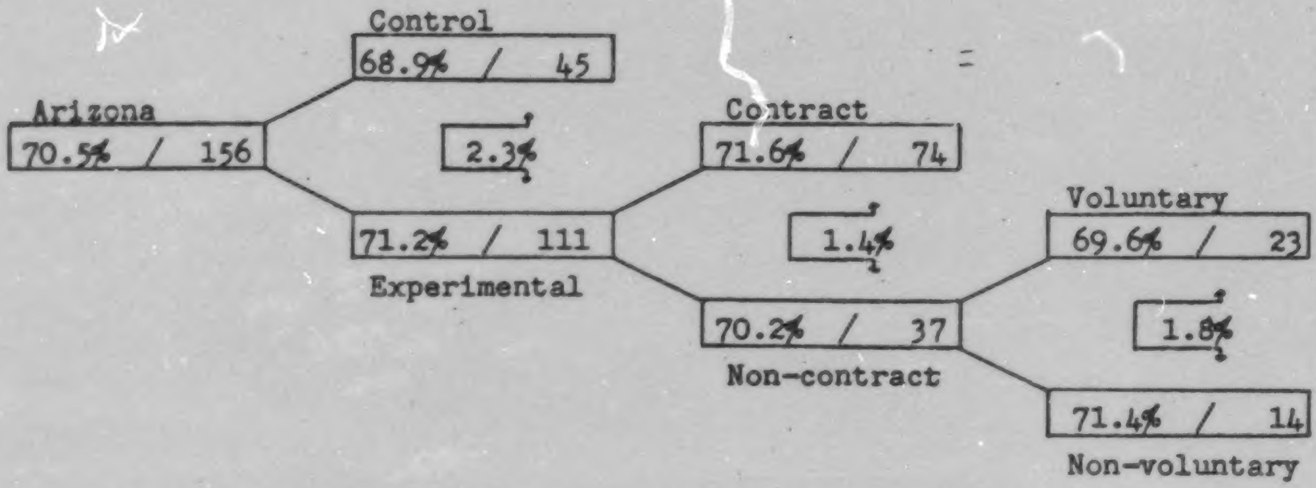
Seventeen percent of the combined study sample said MAP was no help in this area. (Fifty-six percent had said it was a lot of help.)

A somewhat differently worded item provided prisoners, at the time of release, opportunity to respond whether they believed MAP had or would have increased, reduced, or produced no effect on their length of prison stay. Only three percent of Wisconsin cases and four percent of Arizona cases claimed MAP delayed or would have delayed their release. Results were otherwise quite similar on the two items, except that lower threshold for endorsement on the second item (i.e., merely "reduced," rather than helped "a lot") substantially increased the proportion of Wisconsin prisoners who agreed, whereas, in Arizona, prisoners who agreed it had any effect were likely to claim the effect was large.

21 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE TO SPEED HIS PRISON RELEASE


Control
33.3% / 45
Wisconsin
42.9% / 177
12.9%
Contract
37.9% / 66
46.2% / 132
Experimental
16.6%
Voluntary
50.0% / 32
54.5% / 66
Non-contract
8.8%
58.8% / 34
Non-voluntary


Control
68.9% / 45
Arizona
70.5% / 156
2.3%
Contract
71.6% / 74
71.2% / 111
Experimental
1.4%
Voluntary
69.6% / 23
70.2% / 37
Non-contract
1.8%
71.4% / 14
Non-voluntary

BELIEF IN MAP'S EFFICACY FOR LOWERING RECIDIVISM

WISCONSIN AND ARIZONA

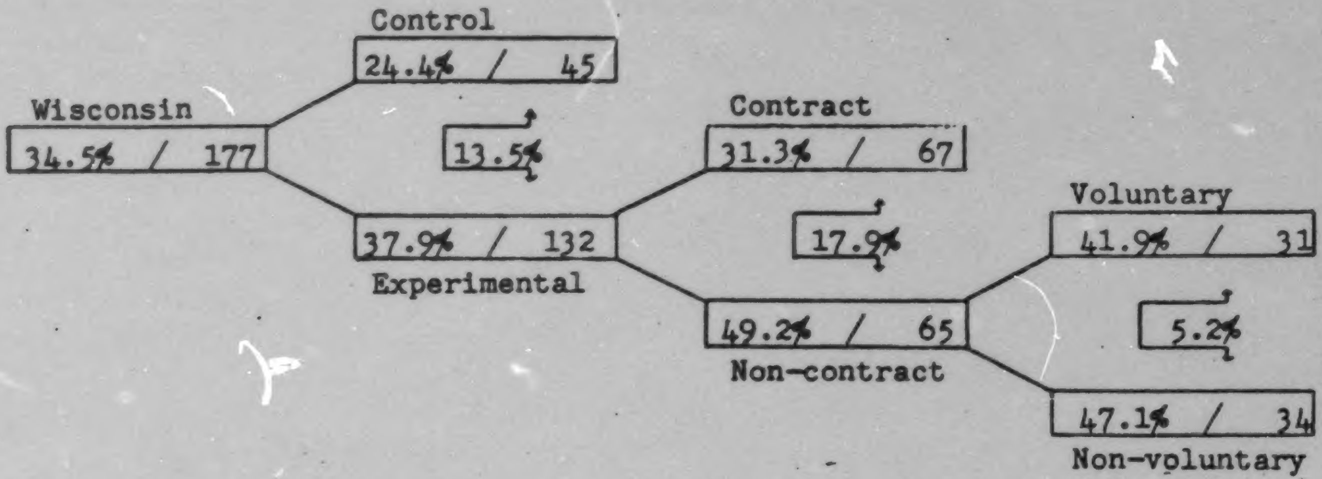
"Helping me stay out of prison in the future" ranked fourth in both Wisconsin and Arizona on prisoners' responses that MAP had helped or would have helped them a lot. In Wisconsin where 34% of the study sample endorsed this MAP effect, greater faith was again exhibited among experimentals than controls, and among non-contract experimentals than those with contracts.

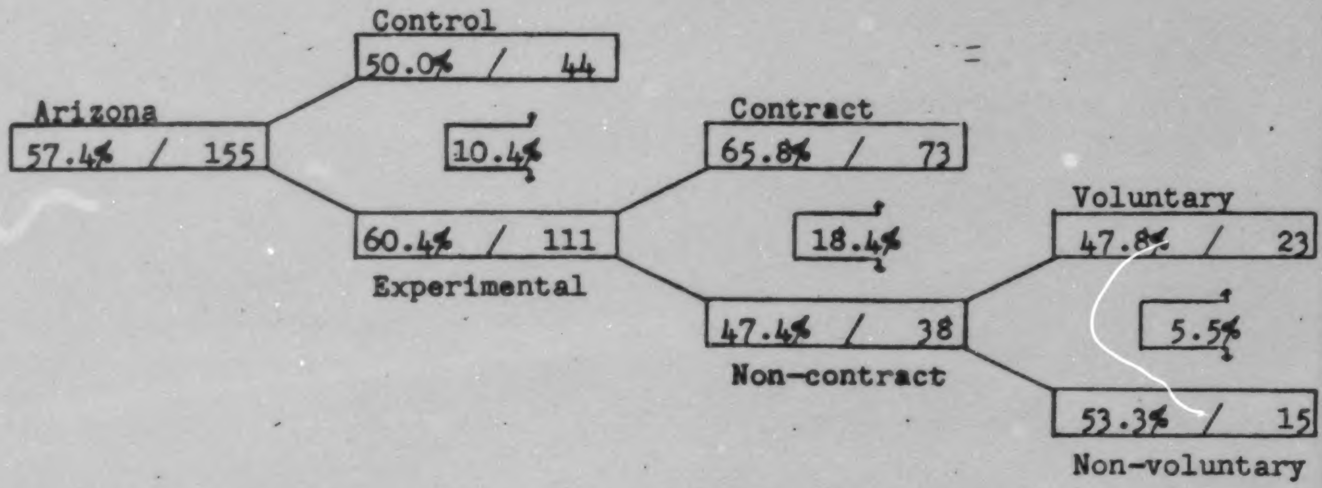
In Arizona, where 57% endorsed the item, experimentals were 10% more likely to agree than controls, and this difference was attributable to greater belief among the contract experimentals (difference = 18 percentage points, $X^2 = 2.78$; $p < .10$).

Twenty-six percent of prisoners in the combined Wisconsin and Arizona study sample claimed MAP was "no help" relative to staying out of prison. (Forty-six percent had said it was "a lot" of help.)

22 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN HELPING HIM STAY OUT OF PRISON


Wisconsin
34.5% / 177
Control
24.4% / 45
13.5%
Experimental
37.9% / 132
Contract
31.3% / 67
17.9%
Non-contract
49.2% / 65
Voluntary
41.9% / 31
5.2%
Non-voluntary
47.1% / 34


Arizona
57.4% / 155
Control
50.0% / 44
10.4%
Experimental
60.4% / 111
Contract
65.8% / 73
18.4%
Non-contract
47.4% / 38
Voluntary
47.8% / 23
5.5%
Non-voluntary
53.3% / 15

BELIEF IN MAP'S EFFECT ON PRISONER MORALE

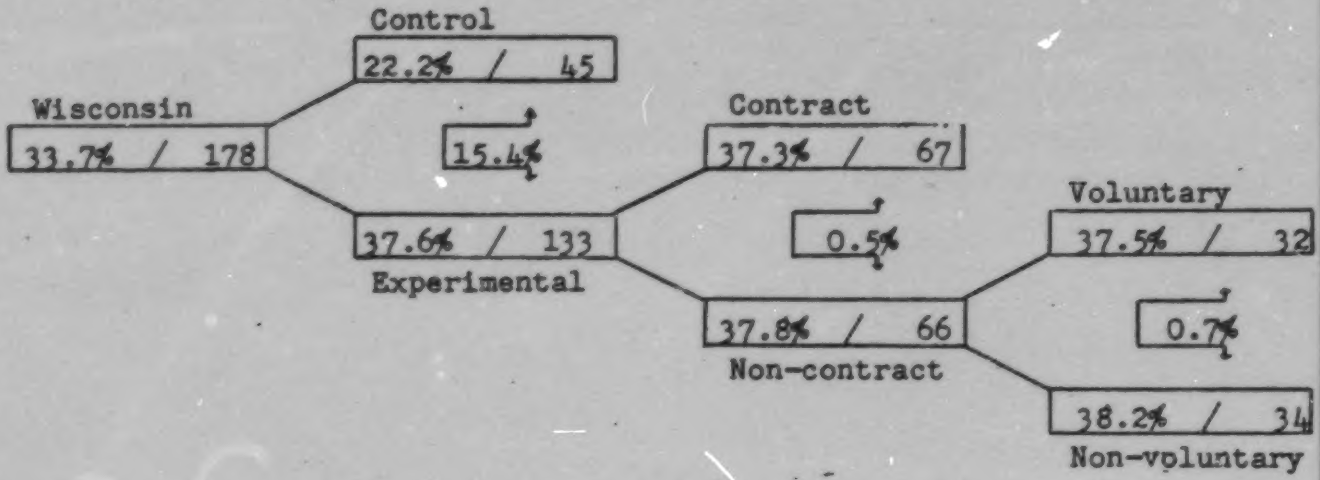
WISCONSIN AND ARIZONA

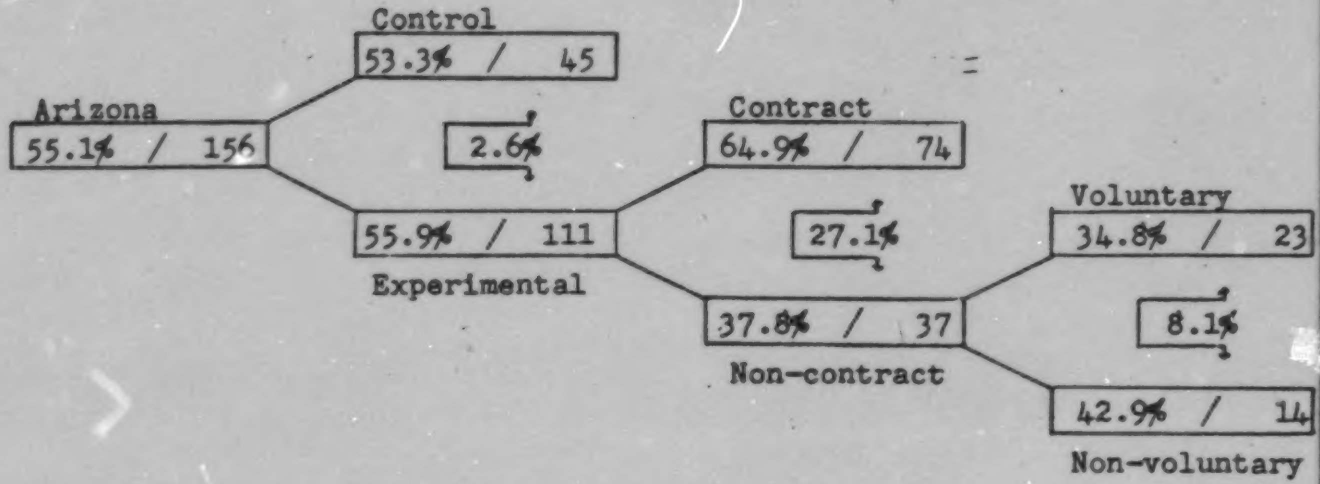
One-third of Wisconsin prisoners (34%) and over half of those in Arizona (55%) agreed that MAP agreements were a lot of help in "getting myself more interested and working harder," placing this form of perceived value fifth in both states. In Wisconsin, experimentals were more likely ($X^2 = 2.90$; $p < .10$) than controls to agree, and differences among experimental subsamples were quite slight. In Arizona, experimental and control endorsements were at the same level, but contract and non-contract experimentals diverged markedly from one another ($X^2 = 6.25$; $p < .025$).

Nineteen percent of subjects in the combined study sample claimed MAP was or would have been "no help" relative to their own interest and effort. (Forty-four percent had said it was "a lot" of help.)

23 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN GETTING HIM INTERESTED IN WORKING HARDER


Wisconsin
33.7% / 178
Control
22.2% / 45
15.4%
37.6% / 133
Experimental
Contract
37.3% / 67
0.5%
37.8% / 66
Non-contract
Voluntary
37.5% / 32
0.7%
38.2% / 34
Non-voluntary


Arizona
55.1% / 156
Control
53.3% / 45
2.6%
55.9% / 111
Experimental
Contract
64.9% / 74
27.1%
37.8% / 37
Non-contract
Voluntary
34.8% / 23
8.1%
42.9% / 14
Non-voluntary

BELIEF IN MAP'S EFFICACY FOR IMPROVED JOB OPPORTUNITY

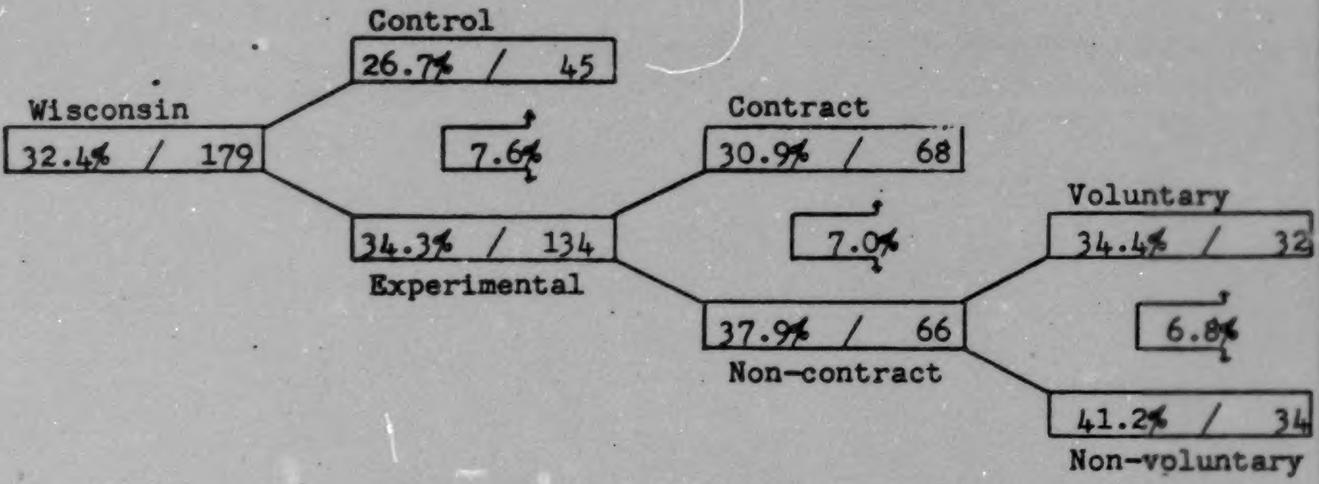
WISCONSIN AND ARIZONA

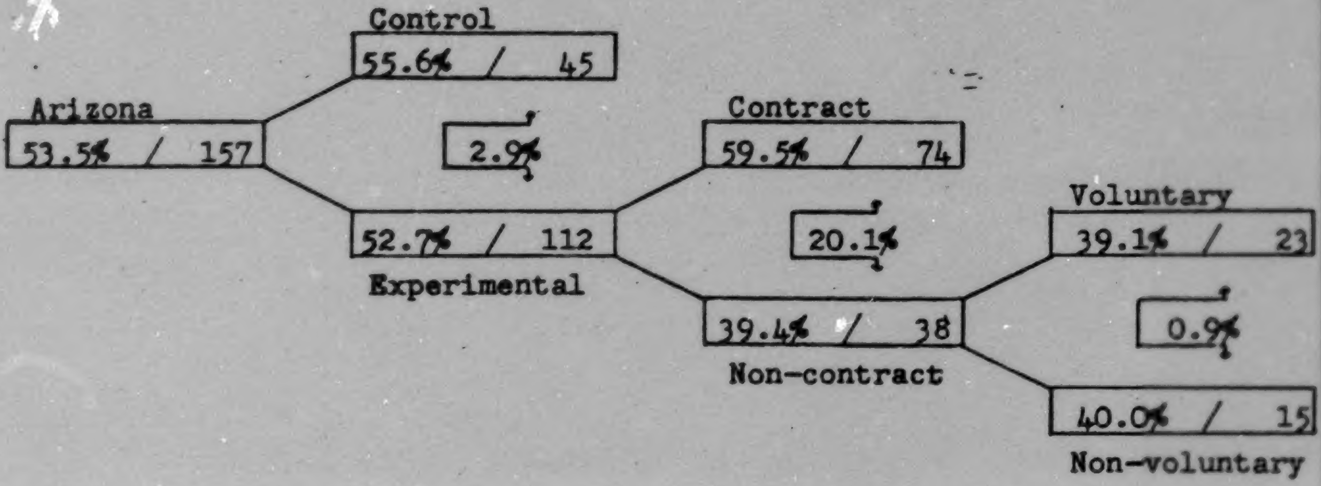
About one-third of Wisconsin prisoners (32%) and one-half of Arizona prisoners (54%) expressed a belief near the time of their release that a MAP agreement made or would have made "a lot" of difference in "improving my chance of getting a good job after release." Within the two states, differences among comparison groups were slight except that, in Arizona, non-contract experimentals were less likely to share such a belief, being 20 percentage points lower than contract experimentals ($X^2 = 3.26$; $p < .10$).

The item placed sixth on level of endorsement in both states, with 24% of the combined study sample claiming MAP was "no help" relative to job prospects after release. (Forty-two percent said it was "a lot" of help.)

24RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN OBTAINING A GOOD JOB AFTER RELEASE


Wisconsin
32.4% / 179
Control
26.7% / 45
7.6%
34.3% / 134
Experimental
Contract
30.9% / 68
7.0%
37.9% / 66
Non-contract
Voluntary
34.4% / 32
6.8%
41.2% / 34
Non-voluntary


Arizona
53.5% / 157
Control
55.6% / 45
2.9%
52.7% / 112
Experimental
Contract
59.5% / 74
20.1%
39.4% / 38
Non-contract
Voluntary
39.1% / 23
0.9%
40.0% / 15
Non-voluntary

BELIEF IN MAP AS ANTIDOTE AGAINST "HARD TIME"

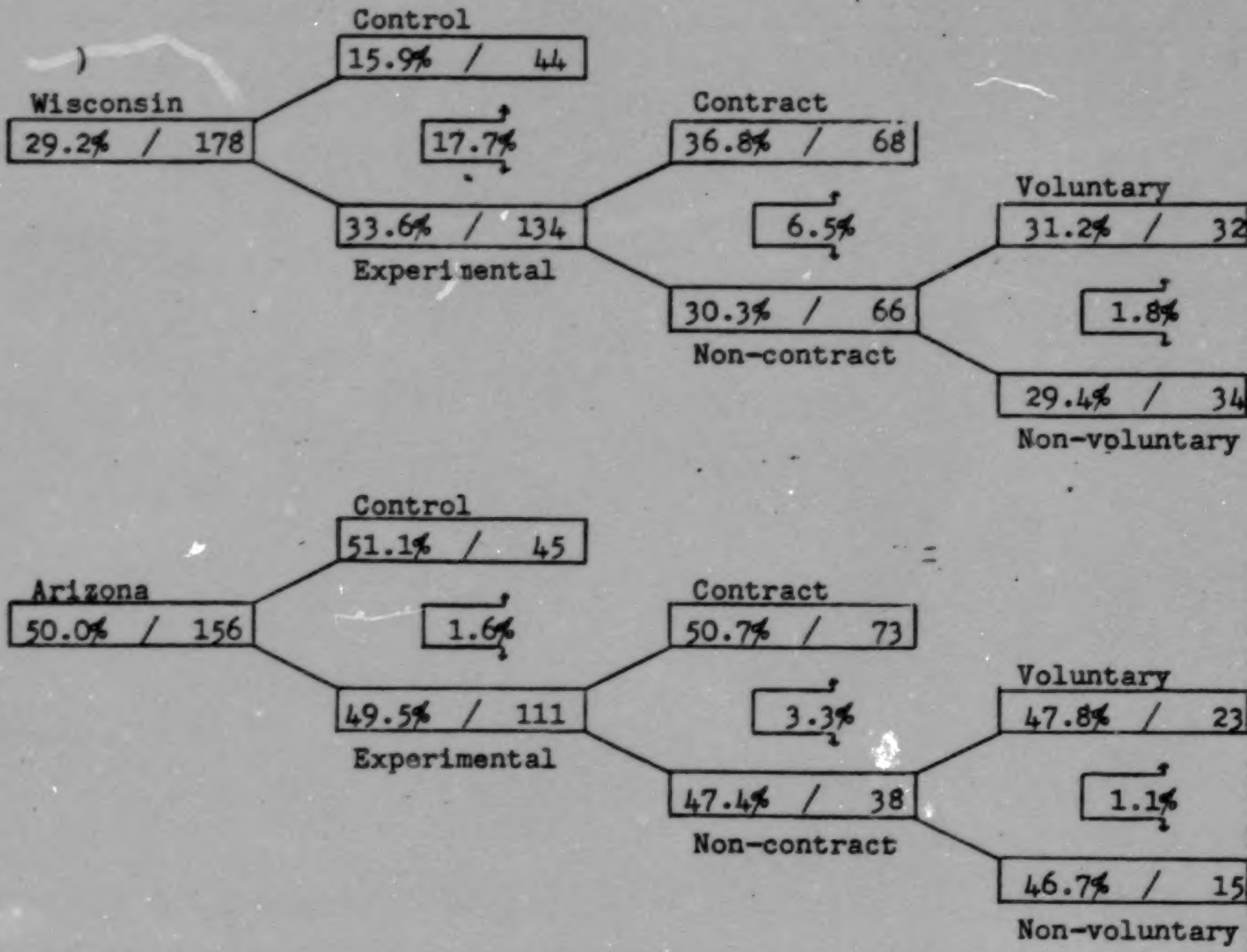
WISCONSIN AND ARIZONA

Ranking seventh among areas of potential MAP assistance in both states was "making my time in prison pass easier," endorsed as helping "a lot" by 29% of Wisconsin prisoners and 50% of Arizona prisoners. All Arizona comparison groups were equally likely to endorse the item.

Twenth-seven percent of prisoners in the combined study sample saw MAP as "no help" relative to ease of passage of time in prison. (Thirty-nine percent said it was "a lot" of help.)

25 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN MAKING HIS PRISON TIME PASS EASIER


Wisconsin
29.2% / 178
Control
15.9% / 44
17.7%
33.6% / 134
Experimental
Contract
36.8% / 68
6.5%
30.3% / 66
Non-contract
Voluntary
31.2% / 32
1.8%
29.4% / 34
Non-voluntary
Arizona
50.0% / 156
Control
51.1% / 45
1.6%
49.5% / 111
Experimental
Contract
50.7% / 73
3.3%
47.4% / 38
Non-contract
Voluntary
47.8% / 23
1.1%
46.7% / 15
Non-voluntary

BELIEF IN MAP'S VALUE FOR OPERATION OF PRISON PROGRAMS

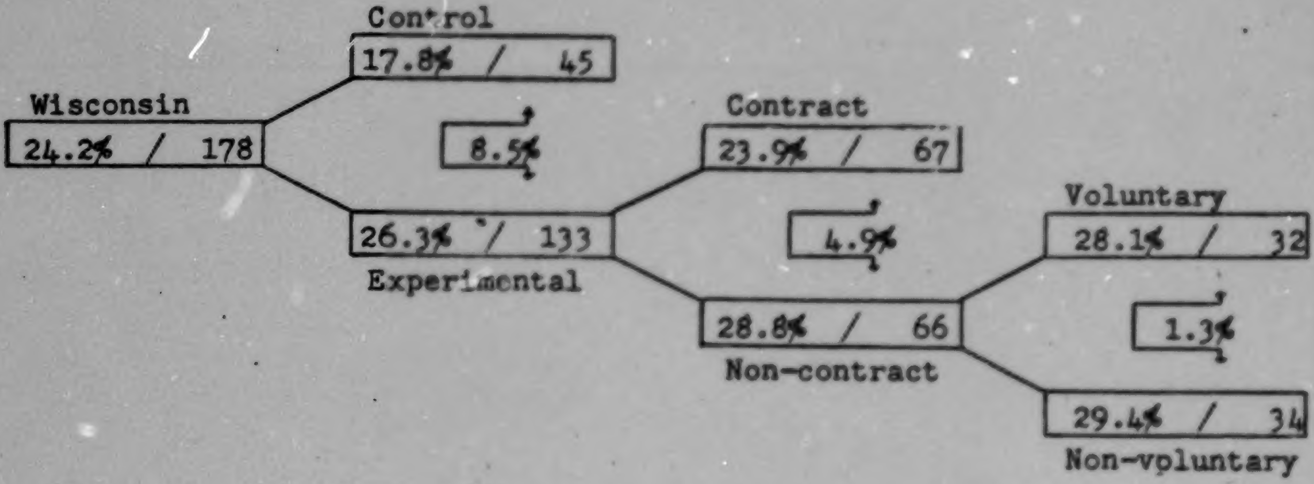
WISCONSIN AND ARIZONA

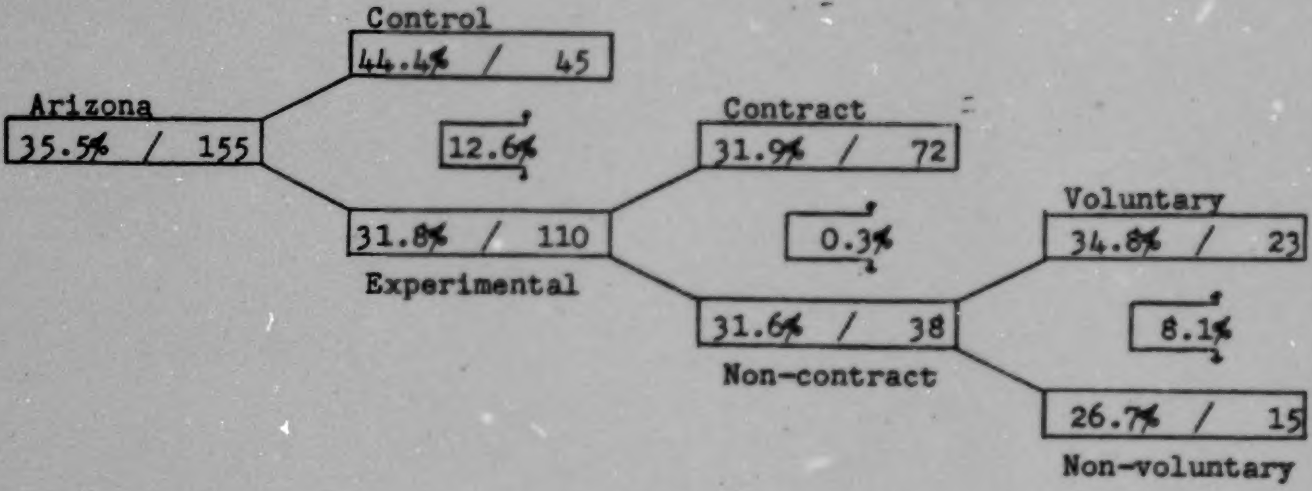
"Making the programs I was already in work better" was in eighth place out of ten in both states, endorsed by 24% in Wisconsin and 36% in Arizona as helping "a lot." Endorsement levels were similar among most comparison groups in each state, but Arizona controls were more likely (13 percentage points) to agree with this item than Arizona experimentals.

Of the items dealing with potential areas of MAP credit thus far discussed, this is the first in which a greater proportion of members of the combined two-state sample claimed MAP was "no help" (34%) than were willing to credit it with being "a lot" of help (29%).

26 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN MAKING PROGRAM HE WAS IN WORK BETTER


Control
17.8% / 45
Wisconsin
24.2% / 178
8.5%
26.3% / 133
Experimental
Contract
23.9% / 67
4.9%
28.8% / 66
Non-contract
Voluntary
28.1% / 32
1.3%
29.4% / 34
Non-voluntary


Control
44.4% / 45
Arizona
35.5% / 155
12.6%
31.8% / 110
Experimental
Contract
31.9% / 72
0.3%
31.6% / 38
Non-contract
Voluntary
34.8% / 23
8.1%
26.7% / 15
Non-voluntary

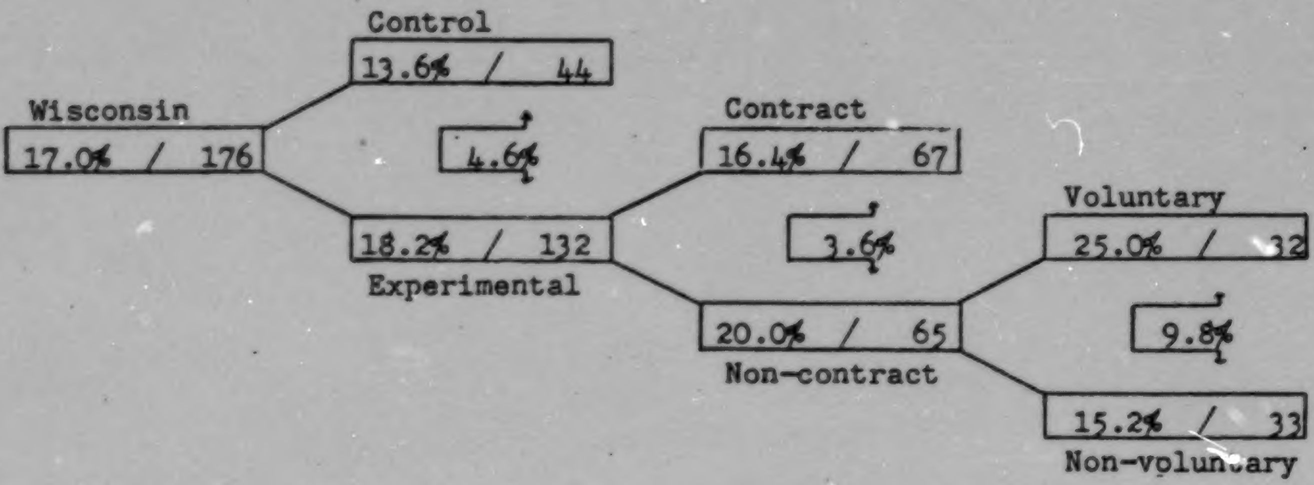
BELIEF IN MAP'S BENEFIT RELATIVE TO STAFF INTEREST

WISCONSIN AND ARIZONA

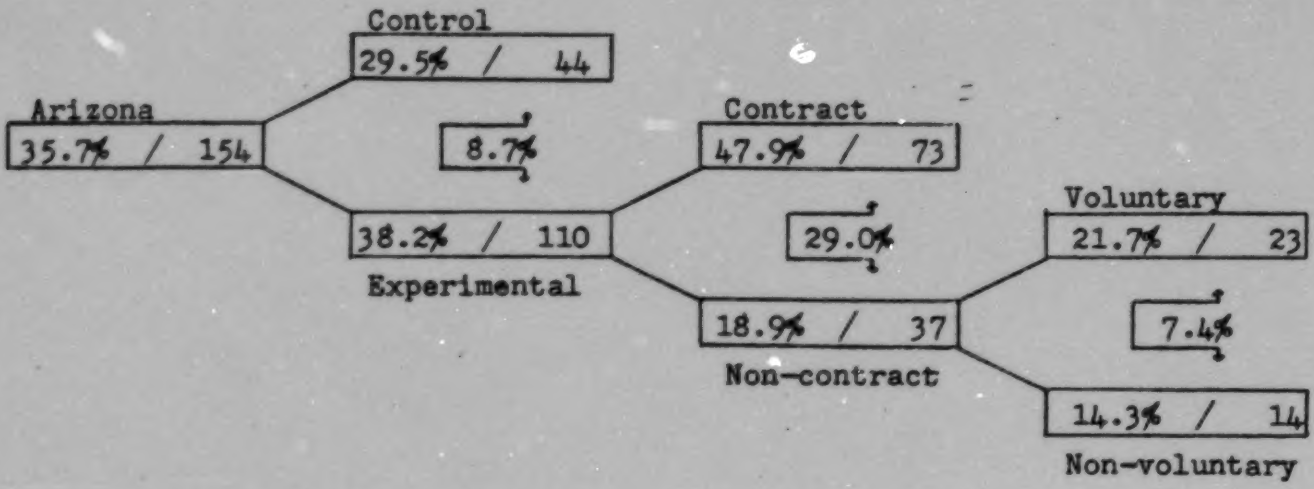
Next to last in level of endorsement among areas of possible MAP benefit in both states was "getting prison staff more interested in helping me." For the combined study sample, 26% claimed MAP was or would have been "a lot" of help in this regard, and 28% claimed it was "no help." In both Arizona (where it was endorsed by 36%) and Wisconsin (where 17% endorsed the item), experimentals were slightly more likely than controls to offer credit, and in Arizona, contract experimentals were significantly more likely than those without contract ($X^2 = 7.58$; $p < .01$) to view MAP as favorably affecting staff interest.

27 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN GETTING PRISON STAFF MORE INTERESTED IN HELPING HIM



Wisconsin
17.0% / 176
Control
13.6% / 44
4.6%
18.2% / 132
Experimental
Contract
16.4% / 67
3.6%
20.0% / 65
Non-contract
Voluntary
25.0% / 32
9.8%
15.2% / 33
Non-voluntary



Arizona
35.7% / 154
Control
29.5% / 44
8.7%
38.2% / 110
Experimental
Contract
47.9% / 73
29.0%
18.9% / 37
Non-contract
Voluntary
21.7% / 23
7.4%
14.3% / 14
Non-voluntary

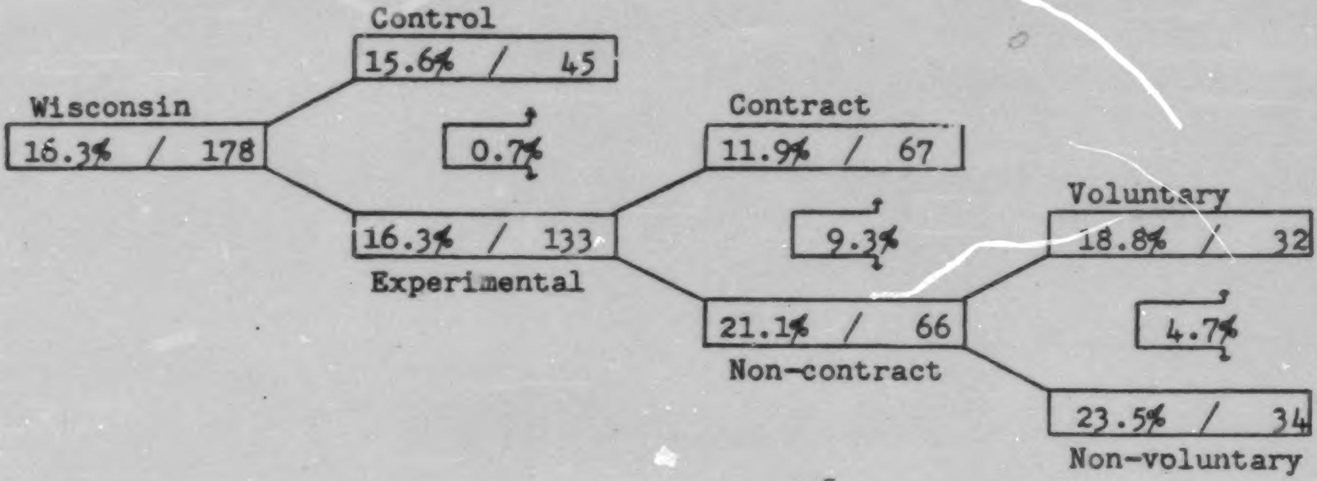
BELIEF IN MAP'S FACILITATION OF ENTRY TO PRISON PROGRAMS

WISCONSIN AND ARIZONA

Lowest in area of overall endorsement, with 26% of the combined study sample viewing MAP as being "a lot" of help, and 48% seeing it as "no help," was "getting me into prison programs I couldn't have gotten without MAP." Only 16% of the prisoners in Wisconsin endorsed this item, and contract experimentals were the subsample least likely to perceive MAP as helping "a lot"--only 12% agreed. In Arizona, where 36% credited MAP as helping "a lot," contract experimentals were the group most likely to agree (40%), and experimentals without contract the group lease likely to agree (26%).

28 RELEASE

PRISONER BELIEVED MAP DID OR WOULD HAVE MADE A LOT OF DIFFERENCE IN ENTERING PRISON PROGRAMS HE COULDN"T OTHERWISE OBTAIN


Wisconsin
16.3% / 178
Control
15.6% / 45
0.7%
16.3% / 133
Experimental
Contract
11.9% / 67
9.3%
21.1% / 66
Non-contract
Voluntary
18.8% / 32
4.7%
23.5% / 34
Non-voluntary

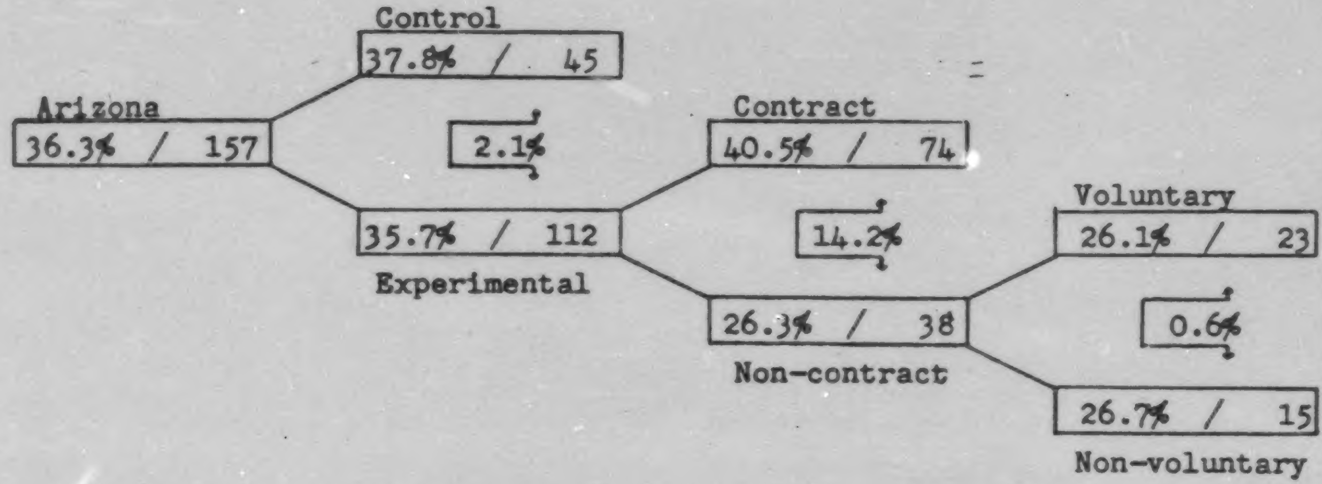

Arizona
36.3% / 157
Control
37.8% / 45
2.1%
35.7% / 112
Experimental
Contract
40.5% / 74
14.2%
26.3% / 38
Non-contract
Voluntary
26.1% / 23
0.6%
26.7% / 15
Non-voluntary

TABLE 3

MAP IN RETROSPECT: PRISONER BELIEFS ABOUT AREAS OF BENEFIT

WISCONSIN AND ARIZONA

BELIEFS ABOUT MAP
COMBINED STUDY SAMPLE

	ITEM	AMOUNT OF HELP A LOT	 NONE
1.	Certainty of release date	71%	9%
2.	Plans for outside	60	11
3.	Earlier release	56	17
4.	Staying out of prison	46	26
5.	Improved prisoner morale	44	19
6.	Job prospects after release	42	24
7.	Easier passage of time	39	27
8.	Better working prison programs	29	34
9.	Improved staff interest	26	28
10.	Better access to prison programs	26	48

The ten areas of potential MAP credit are listed above with order determined merely on the basis of the proportion of prisoners who saw MAP as being "a lot" of help. This order was nearly identical for Wisconsin and for Arizona, and Arizona subjects were more enthusiastic than those in Wisconsin about MAP benefits in most of the separate areas.

Faith in MAP's ability to help "a lot" was displayed across all subsamples in each state by a majority of prisoners only on the top-listed item--providing certainty of release date, and by a simple majority of the full study in each state for the first and the second item--facilitating plans and arrangements outside. No item beyond the third--earlier release from prison--enlisted support of the majority of any subsample in Wisconsin, and no item beyond it secured support by majorities in every Arizona subsample.

The overall level of support for items 4 through 7 differed little from one another within the individual states, securing endorsement from roughly one-third of the Wisconsin prisoners and roughly one-half of those in Arizona. These items dealt with improvement of prisoner morale and easier passage of prison time, and with lowering recidivism likelihood and improved prospects for a good job outside. No item beyond 7 secured majority support from any Arizona subsample.

The bottom three items each received support from about one-third of the Arizona prisoners, and by less than one-fourth of the Wisconsin prisoners. These items dealt with the improved effect of MAP on prison programs--both entry to them and operation of them--and with the enlistment of prison staff interest in helping prisoners.

In general, differences in level of item endorsement between the two states--Wisconsin and Arizona--and between the two basic conditions--experimental and control--are overshadowed by consensus among the subgroups about the ways in which MAP was relatively most likely to be of help, and relatively least likely. Thus, despite the differences in their circumstances (and, consequently, in whether their response represented a perception or a speculation) experimentals and controls agreed quite closely in their rankings of the ten content areas from most to least beneficial--in Wisconsin, the correlation was .924 and in Arizona it was .867. Agreement between the full study samples for the two states yielded a rank order correlation of .939. MAP was, then, widely viewed as having greatest promise for establishing a more certain release date, facilitating earlier release, and making it more convenient to plan arrangements in the community, and was seen as having considerably less utility for improving access to prison programs on the operation of such programs, or for eliciting greater interest in prisoners from staff.

PRISONER BELIEFS ABOUT MAP ASSISTANCE

COMBINED WISCONSIN/ARIZONA
STUDY SAMPLE

"How much difference do you think getting and holding onto a MAP agreement made in your case? (If you never had a MAP agreement or dropped out of one, answer the questions in terms of how much difference you think getting and holding onto an agreement would have made.)"

	Amount of Difference MAP Agree- Made or Would Have Made (RESPONSE DISTRIBUTION)			
	none	a little	a fair amount	a lot
15. Getting prison staff more interested in helping me	(1) 28%	(2) 24%	(3) 22%	(4) 26%
16. Getting myself more interested and working harder	(1) 19%	(2) 14%	(3) 23%	(4) 44%
17. Getting me into prison programs I couldn't have gotten without MAP	(1) 48%	(2) 11%	(3) 15%	(4) 26%
18. Making the programs I was already in work better	(1) 34%	(2) 17%	(3) 20%	(4) 29%
19. Making me more certain of my release date	(1) 9%	(2) 7%	(3) 13%	(4) 71%
20. Helping me plan & make arrangements outside because I knew when I'd be going home	(1) 11%	(2) 10%	(3) 19%	(4) 60%
21. Making my time in prison pass easier	(1) 27%	(2) 16%	(3) 18%	(4) 39%
22. Getting me out of prison earlier	(1) 17%	(2) 12%	(3) 15%	(4) 56%
23. Improving my chances of getting a good job after release	(1) 24%	(2) 15%	(3) 19%	(4) 42%
24. Helping me stay out of prison in the future	(1) 26%	(2) 12%	(3) 16%	(4) 46%

EXPECTED EASE IN LOCATING WORK

WISCONSIN

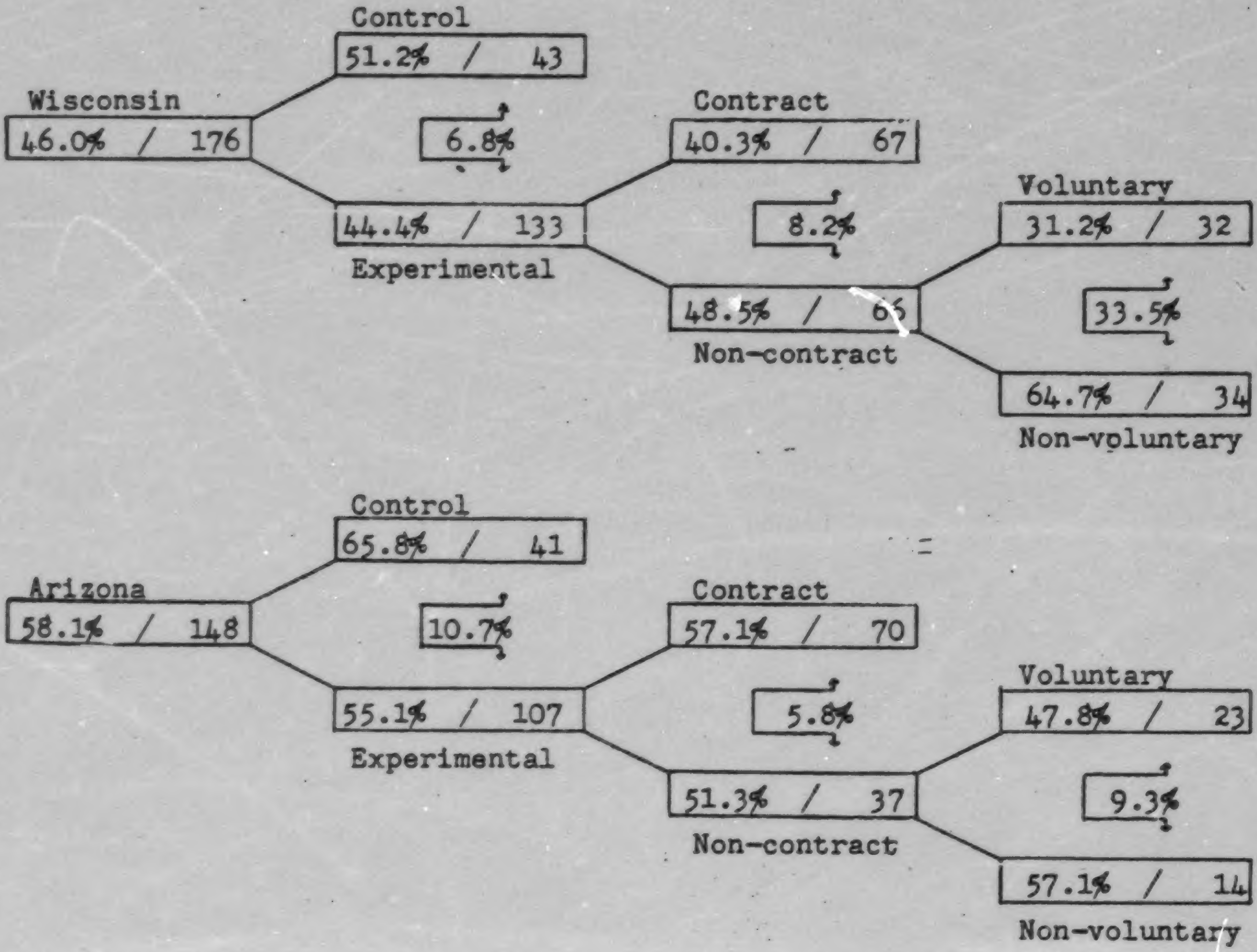
At the time of release preparations subjects were asked to report on the best job they believed they would probably obtain within the first six months after release; 46% of the Wisconsin prisoners anticipated that it would take less than a week in the community to obtain the job they sought, and a majority of these claimed the job was already waiting for them. Controls were slightly (7 percentage points) more likely than experimentals to anticipate rapid placement, and non-contract experimentals were slightly (8 percentage points) more likely than those completing contracts to show this optimism. A substantial (34 percentage point) and statistically significant difference ($X^2 = 6.11$; $p < .02$) existed between voluntary and non-voluntary contract cases, with those who were denied or removed from contract programming twice as likely as those who declined or dropped from participation to expect early placement; this latter finding runs contrary to expectation, since one would assume voluntary drops would see themselves as less needy than those who attempted but failed to enter or retain contract programming.

ARIZONA

In Arizona, 58% of prisoners approaching release either expected to obtain in less than a week, or claimed already to possess the job they sought. As in Wisconsin, controls were somewhat (11 percentage points) more likely than experimentals to anticipate early placement, but in Arizona contract cases were slightly (6 percentage points) more likely than non-contract experimentals to show optimism. Again, as in Wisconsin, non-voluntary drops were more (9 percentage points) likely than cases who voluntarily declined contract involvement to expect early placement.

29 RELEASE

PRISONER ANTICIPATED ONE WEEK OR LESS AFTER RELEASE TO OBTAIN BEST EARLY JOB


Control
51.2% / 43
Wisconsin
46.0% / 176
6.8%
Contract
40.3% / 67
44.4% / 133
Experimental
8.2%
Voluntary
31.2% / 32
48.5% / 66
Non-contract
33.5%
64.7% / 34
Non-voluntary
Control
65.8% / 41
Arizona
58.1% / 148
10.7%
Contract
57.1% / 70
55.1% / 107
Experimental
5.8%
Voluntary
47.8% / 23
51.3% / 37
Non-contract
9.3%
57.1% / 14
Non-voluntary

HOURLY WAGES ANTICIPATED IN EARLY JOB

WISCONSIN

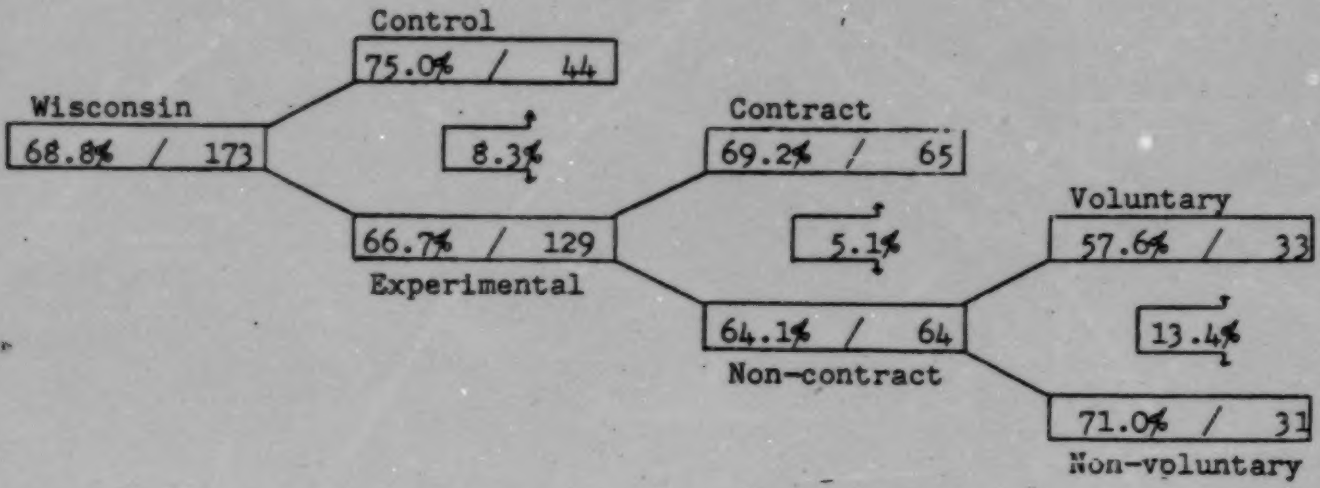
Two-thirds (68%) of Wisconsin prisoners indicated on the release questionnaire that they expected to earn $3.00 per hour or more in a job obtained during the early months in the community. Controls were 8 percentage points more likely to anticipate reaching this earning level than were experimentals, and experimentals completing contract were slightly (5 percentage points) more optimistic than those who had failed to obtain or who dropped from MAP programming. Among these non-contract experimentals, those non-voluntarily removed had higher (13 percentage points) expectations of reaching $3.00 per hour than those who had themselves decided against MAP involvement.

ARIZONA

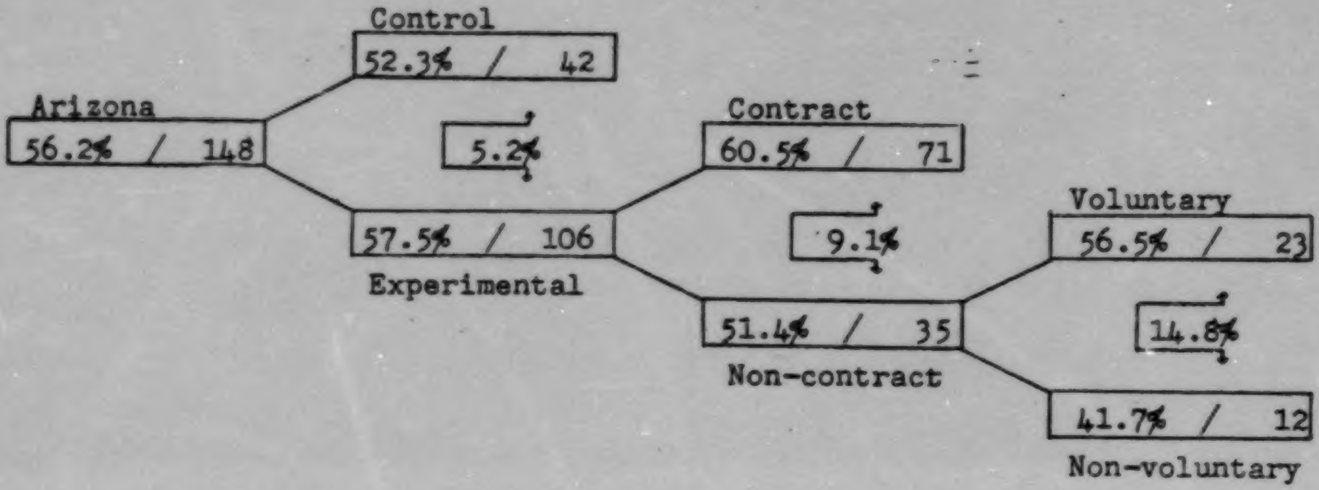
A majority (56%) of Arizona prisoners believed they would attain $3.00 per hour or more in earnings during their first months after release. Experimentals were slightly (5 percentage points) more optimistic than controls, experimentals completing contract (9 percentage points) more optimistic than those without, and voluntary MAP drops (15 percentage points) more optimistic than non-voluntary drops. For once, all the differences among comparison groups are in the expected direction (Experimental > Control; Contract > Non-contract; Voluntary > Non-voluntary), although none of the differences is substantial or statistically significant.

30 RELEASE

PRISONER ANTICIPATED $3.00/HR. OR MORE ON BEST EARLY JOB



Wisconsin
68.8% / 173
Control
75.0% / 44
8.3%
Experimental
66.7% / 129
Contract
69.2% / 65
5.1%
Non-contract
64.1% / 64
Voluntary
57.6% / 33
13.4%
Non-voluntary
71.0% / 31



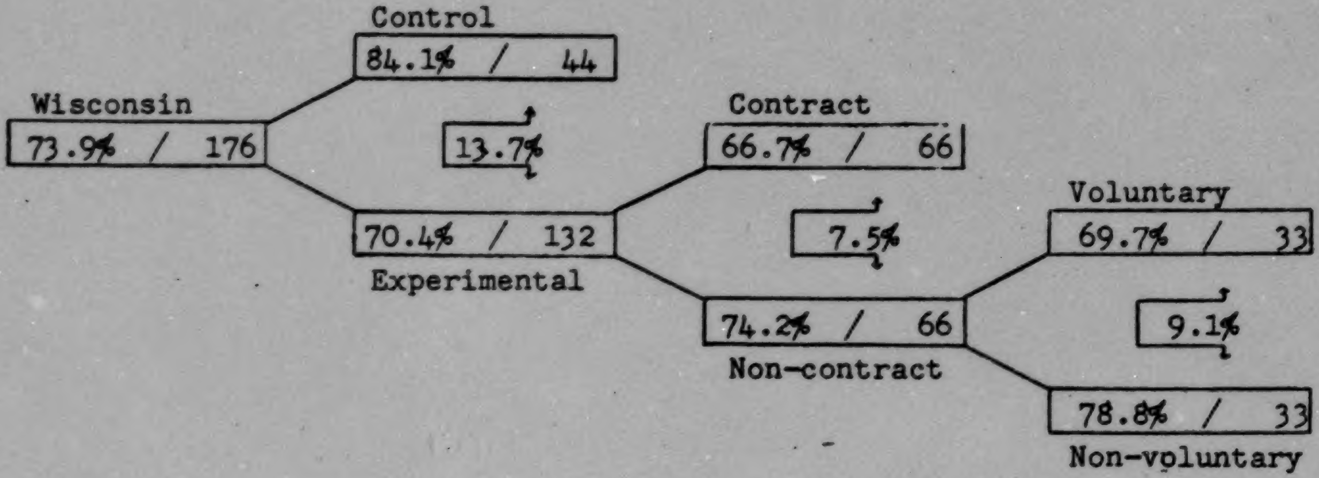
Arizona
56.2% / 148
Control
52.3% / 42
5.2%
Experimental
57.5% / 106
Contract
60.5% / 71
9.1%
Non-contract
51.4% / 35
Voluntary
56.5% / 23
14.8%
Non-voluntary
41.7% / 12

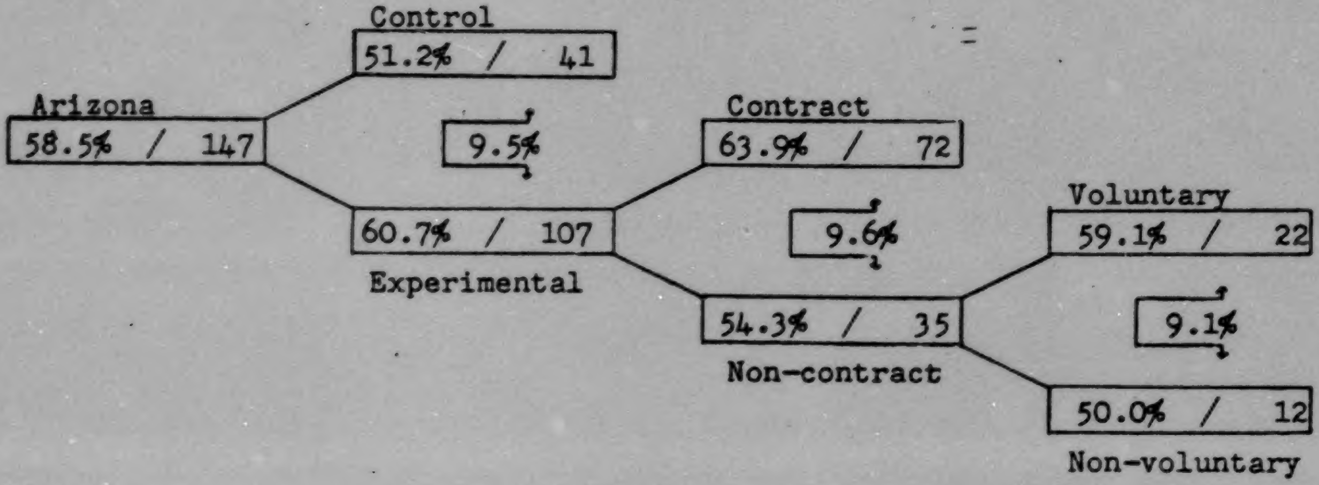
MONTHLY TAKE-HOME EARNINGS ANTICIPATED IN EARLY JOB

WISCONSIN AND ARIZONA

Prisoner expectations on monthly take-home pay from the best job they would obtain paralleled the expectations on hourly wage; though the differences in expectation between experimentals and controls in both states was greater on the monthly earnings expectation, these differences remained short of statistically significant levels.

31 RELEASE
PRISONER ANTICIPATED MONTHLY TAKE-HOME PAY OF $400 OR MORE ON BEST EARLY JOB


Wisconsin
73.9% / 176
Control
84.1% / 44
13.7%
70.4% / 132
Experimental
Contract
66.7% / 66
7.5%
74.2% / 66
Non-contract
Voluntary
69.7% / 33
9.1%
78.8% / 33
Non-voluntary


Arizona
58.5% / 147
Control
51.2% / 41
9.5%
60.7% / 107
Experimental
Contract
63.9% / 72
9.6%
54.3% / 35
Non-contract
Voluntary
59.1% / 22
9.1%
50.0% / 12
Non-voluntary

PRISON WORK EXPERIENCE RELEVANT TO ANTICIPATED JOB AFTER RELEASE

WISCONSIN

Prisoners were asked at the time of release whether the kind of job which they hoped to obtain in the community was one in which they had obtained any work experience during their most recent period of incarceration. Twenty-eight percent of Wisconsin respondents claimed they had some experience, but only 16% claimed work experience in excess of six months. Controls were slightly (5 percentage points) more likely than experimentals to claim any job-relevant prison work experience at all, and were almost twice as likely to claim experience in excess of six months.* Differences between contract and non-contract experimentals were slight, but in a direction favoring those without contract on both the measure of any experience, and the measure of more than six months experience.

ARIZONA

Forty-four percent of Arizona respondents claimed some prison-obtained work experience relevant to the job they hoped to obtain after release. Experimentals were slightly more likely than controls (6 percentage points) to claim any experienc at all, but twice as likely to claim more than six months experience; the latter finding does not reach a statistically significant level. Within the experimental sample, prisoners with contract were significantly more likely ($X^2 = 6.04$; $p < .025$) than those without contracts to claim any relevant experience, and also twice as likely to claim experience in excess of six months.

An identically-worded item had been asked in Arizona at the time of intake testing. At this initial testing only 26% of the study sample members claimed any relevant prison work experience for the job they wished to obtain after release. The overall change between intake and release testing was 18 percentage points, rising from an initial 26% to the final 44%. Experimentals had been 5 percentage points more likely than controls to claim some relevant experience at the time of intake, and this difference had risen by only 1 percentage point at release testing--to 6%. While the rise between intake and release testing was 17 percentage points among controls and 18 among experimentals, the experimental rise was limited entirely to the contract experimental subsample, in which the proportion claiming some relevant experience rose from an initial 28% to a final 55%--a 27 percentage point increase--while the proportion making such a claim among non-contract experimentals was 28% at both the time of intake and the time of release. What appears to have taken place then, over the course of MAP, was a re-distribution of perceived opportunity among the experimental subsamples, but no net increment in the full experimental sample relative to controls.

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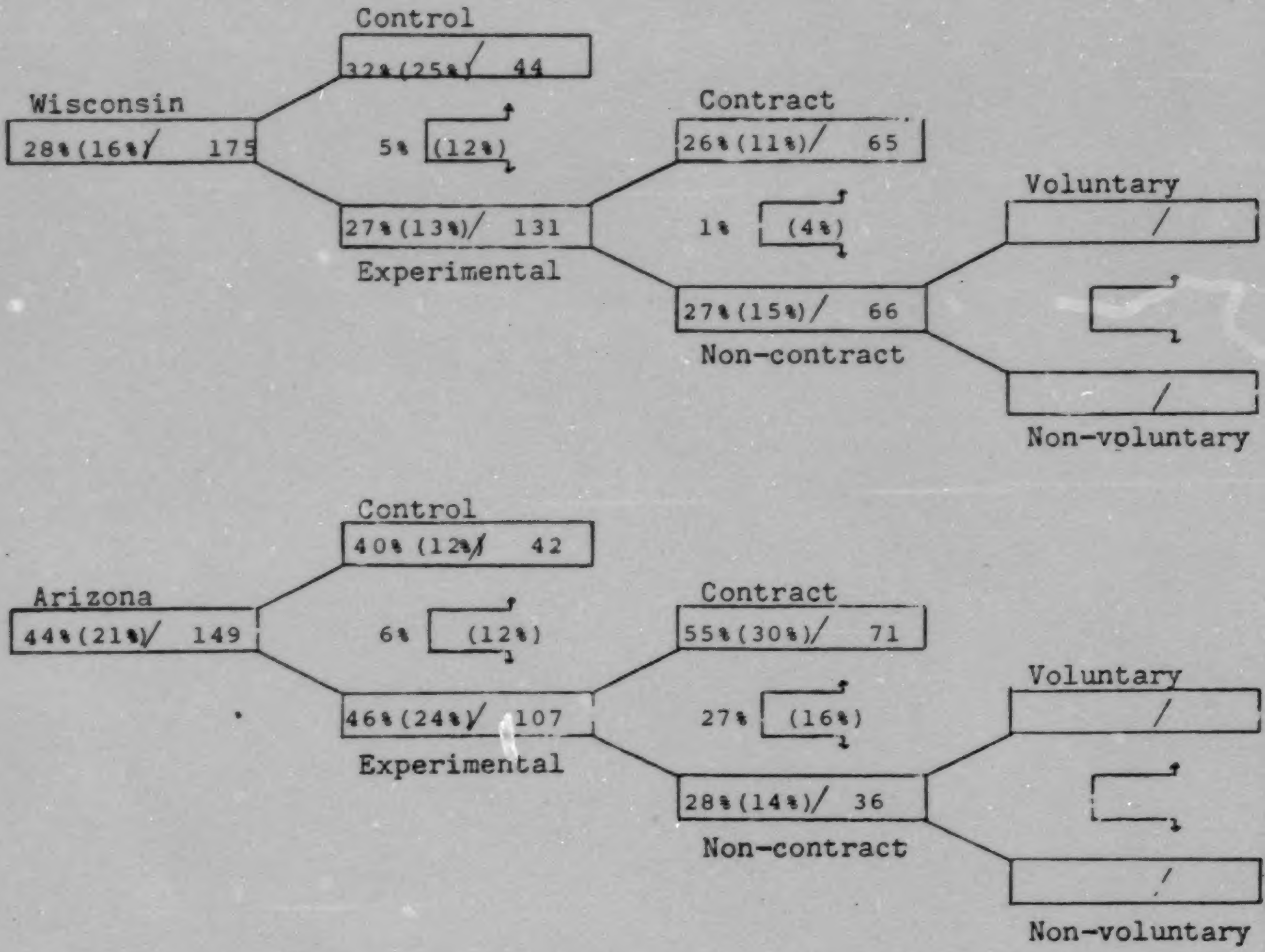
*Twenty-five percent of controls and 13% of experimentals claimed more than 6 months experience. The difference reaches trend significance ($X^2 = 2.70$; $p < .10$).

The pattern of change is somewhat different when prison work experience exceeding six months, rather than any experience at all, is made the focus of examination, or the issue becomes one of duration rather than entry. On this second measure (the proportion of prisoners claiming more than six months relevant experience) the overall change was 11 percentage points--rising from an initial 10% to a final 21%. The proportion making such a claim doubled among both controls (rising from 6% at intake to 12% at release) and among experimentals (rising from 12% at intake to 24% at release), spreading the gap between experimentals and controls to 12 percentage points, and the increase among experimentals was again limited almost exclusively to those under contract. (See table following.)

33 RELEASE
PRISONER CLAIMED PRISON WORK EXPERIENCE RELEVANT TO JOB EXPECTED AFTER RELEASE


Control
32%(25%)/ 44
Wisconsin
28%(16%)/ 175
5% (12%)
Contract
26%(11%)/ 65
27%(13%)/ 131
Experimental
1% (4%)
Voluntary
/
27%(15%)/ 66
Non-contract
/
Non-voluntary
Control
40%(12%)/ 42
Arizona
44%(21%)/ 149
6% (12%)
Contract
55%(30%)/ 71
46%(24%)/ 107
Experimental
27% (16%)
Voluntary
/
28%(14%)/ 36
Non-contract
/
Non-voluntary

Percent without parentheses = Any prison work experience at all.
Percent in parentheses = More than 6 months prison work experience.

PRISON VOCATIONAL TRAINING RELEVANT TO ANTICIPATED RELEASE JOB

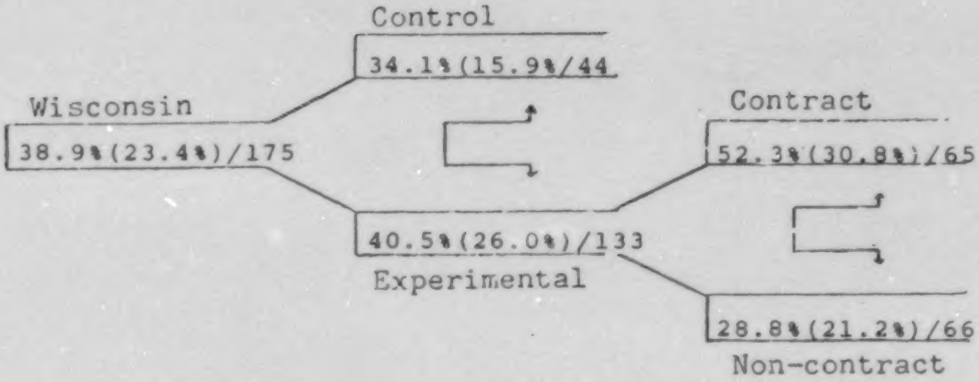
WISCONSIN

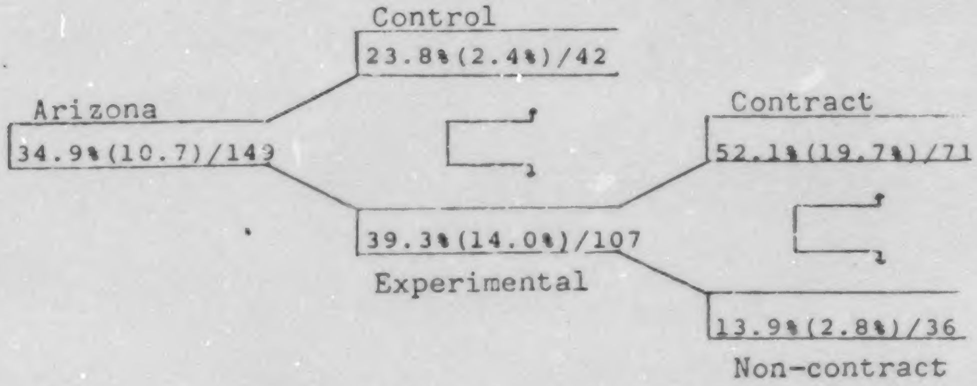
In an item paralleling that for prison work experience, prisoners were asked about any coursework taken during their prison stay that had helped to prepare them for the kind of job they would seek after release.

Thirty-nine percent of Wisconsin prisoners claimed during testing near the date of their release that they had obtained job-relevant coursework, and 24% claimed coursework of more than six months duration. Experimentals were slightly (6 percentage points) more likely than controls to claim any coursework, and more likely (10 percentage points) to claim coursework lasting more than six months.

Experimentals under contract were substantially (24 percentage points) more likely than those without contract to claim some coursework ($X^2 = 6.58$; $p < .025$), but not markedly more likely to claim the lengthier period of coursework. No exactly comparable item is available from Wisconsin intake testing to determine whether the difference between contract and non-contract experimentals was a pre-existing one (i.e., that the contract negotiation process selectively screened those with coursework toward contract and those without away from contract) or whether it was established during the implementation of MAP programming. However, from responses to a fairly similar question asked at intake, no evidence is available of pre-existing differences between these experimental subsamples, or between experimentals and controls.

34 RELEASE
PRISONER CLAIMED PRISON COURSEWORK WAS RELEVANT TO JOB EXPECTED AFTER RELEASE


Control
34.1%(15.9%/44
Wisconsin
38.9%(23.4%)/175
Contract
52.3%(30.8%)/65
40.5%(26.0%)/133
Experimental
28.8%(21.2%)/66
Non-contract


Control
23.8%(2.4%)/42
Arizona
34.9%(10.7)/149
Contract
52.1%(19.7%)/71
39.3%(14.0%)/107
Experimental
13.9%(2.8%)/36
Non-contract

Percent without parentheses = Any course work at all.
Percent in parentheses = Any course work exceeding six months.

ARIZONA

Arizona intake and release testings did contain identically-worded items about the relationship of prison coursework to job sought after release. Seventeen percent at the time of initial testing, and this doubled, to 35% at the final or release testing. Experimentals initially held a four percentage point advantage over controls, and this grew to a 15 percentage point advantage; even if the initial "boost" is not acknowledged, the final difference fails to reach even a trend level of statistical significance. (X-C difference for 6 months coursework is at $p < .10$; $X^2 = 3.13$).

The initial and final advantage among the experimental over the control sample on job-relevant coursework is isolated entirely to the contract subsample of experimentals. The non-contract subsample started nearly even with controls on intake testing, but fell substantially behind them by the time of release testing by virtue of the fact that no increase at all occurred for this subsample in the proportion claiming some coursework. In consequence, the experimentals under contract at release are dramatically ahead (nearly four times as many claiming some coursework) of experimentals without contract ($X^2 = 13.08$; $p < .001$). (C-N difference for 6 months coursework is at $p < .10$; $X^2 = 4.37$). The phenomenon parallels that found for job-relevant prison work experience, and suggests some robbing of Peter to pay Paul occurring within the experimental group, although in the present example some net advantage of experimentals over controls also results. Whether any such net advantage to experimentals also was achieved through foreclosure of opportunity for controls (i.e., through preferential admission to resources of limited availability) cannot be determined through the data available in the current study. Some data bearing tangentially on this point are, however, to be found in the report section dealing with staff impressions.

TABLE 5

PRISON COURSEWORK
RELEVANT TO ANTICIPATED JOB AFTER RELEASE
RESPONSES ON INTAKE AND RELEASE QUESTIONNAIRES
ARIZONA

	ANY COURSEWORK			6 MOS. + COURSEWORK		
GROUP	INTAKE	RELEASE	DIFF	INTAKE	RELEASE	DIFF
STUDY SAMPLE	17%	35%	+18%	5%	11%	+ 6%
CONTROL	14	24	+10	5	2	- 3
EXPERIMENTAL	18	39	+21	5	14	+ 9
CONTRACT	22	52	+30	7	20	+13
NON-CONTRACT	13	14	+ 1	2	3	+ 1

PRISON PREPARATION FOR JOB ANTICIPATED AFTER RELEASE

WISCONSIN

Prisoners were asked to estimate at the time of release how much of the preparation for the job which they hoped to obtain had taken place during their prison stay, and were offered six alternative responses ranging from "none" through "about half" to "all."

In Wisconsin, 27% of the study sample claimed that all preparations for their post-release job had occurred in prison, 46% claimed at least half of it had occurred there, and 39% claimed that none of the preparation was secured in prison. Differences among experimentals and controls on these measures were extremely slight -- between one and four percentage points.

Contract and non-contract experimentals were equally likely to claim all their preparation for the post-release job had been obtained while inside prison on their current stay, but those with contract were 12 percentage points more likely to claim at least half was obtained there, and 15 percentage points less likely to claim that none of it had been.

ARIZONA

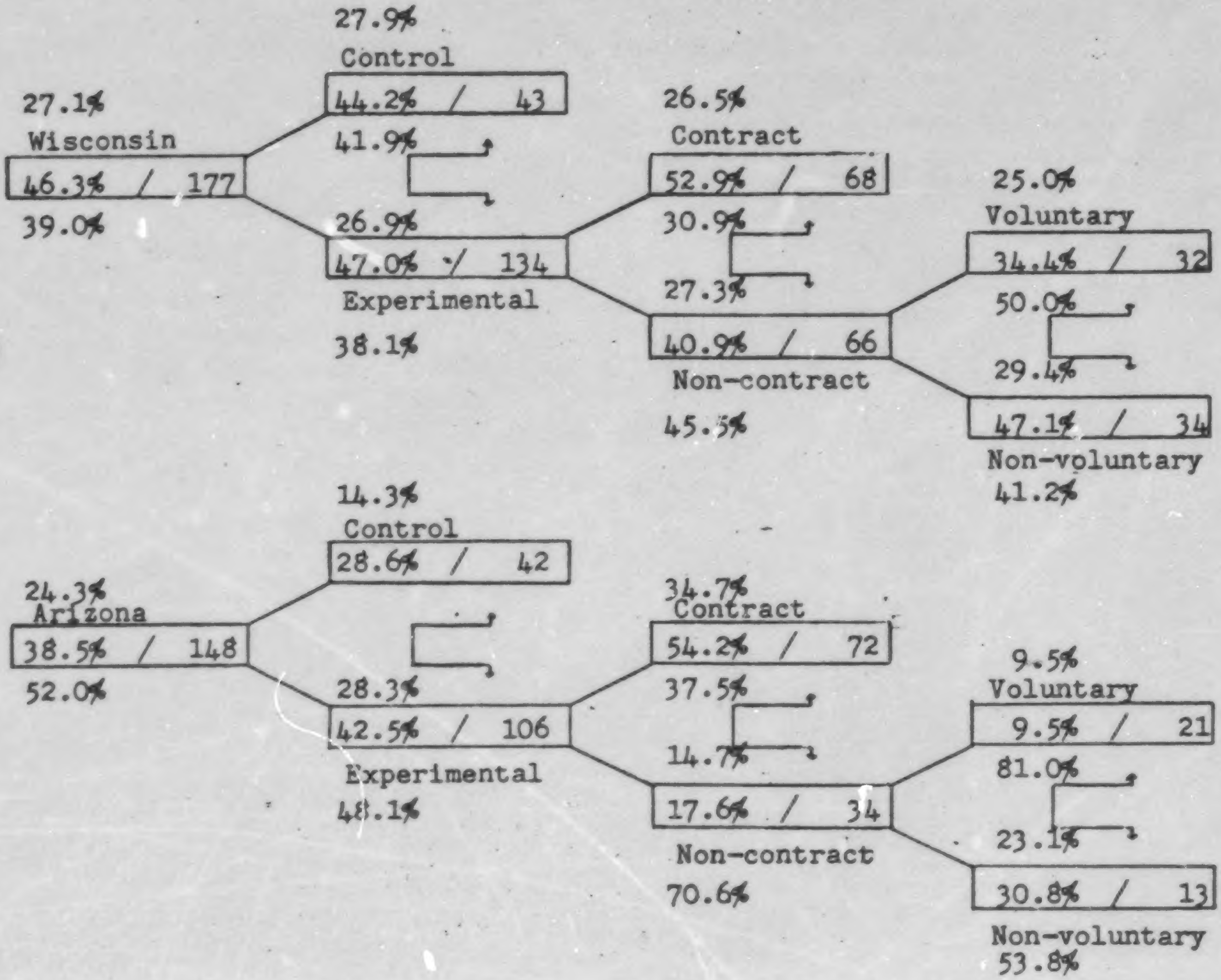
Comparably-worded items at both intake and release testing were available on which to examine responses of Arizona prisoners about existing preparation for the desired job after release.

For the entire study sample, the proportion claiming that all the relevant training they possessed had been obtained during their prison stay climbed from an initial 14% at intake to a final 24% at release -- a rise of 10 percentage points. Similarly, the proportion claiming at least half rose from 21% to 38% -- a 17 percentage point gain, while the proportion claiming no preparation fell from 62% at intake to 52% at release -- a 10 percentage point drop.

At intake, controls were 10 percentage points more likely to claim no relevant prison preparation, and this difference widened to 14 percentage points at release; on claims of half or more preparation inside, controls started at an 8 percentage point disadvantage to experimentals and finished with a 13 percentage point disadvantage; the initial

36 RELEASE

PRISONER'S ESTIMATE OF PROPORTION OF PREPARATION FOR JOB AFTER RELEASE OBTAINED DURING CURRENT PRISON STAY


27.1%
Wisconsin
46.3% / 177
39.0%
27.9%
Control
44.2% / 43
41.9%
26.9%
47.0% / 134
Experimental
38.1%
26.5%
Contract
52.9% / 68
30.9%
27.3%
40.9% / 66
Non-contract
45.5%
25.0%
Voluntary
34.4% / 32
50.0%
29.4%
47.1% / 34
Non-voluntary
41.2%
24.3%
Arizona
38.5% / 148
52.0%
14.3%
Control
28.6% / 42
28.3%
42.5% / 106
Experimental
48.1%
34.7%
Contract
54.2% / 72
37.5%
14.7%
17.6% / 34
Non-contract
70.6%
9.5%
Voluntary
9.5% / 21
81.0%
23.1%
30.8% / 13
Non-voluntary
53.8%

Percent in box: About half or more preparation obtained in prison.
Percent above box: All preparation obtained in prison.
Percent beneath box: None of preparation obtained in prison.

spread on claims of all preparation inside was 3 percentage points, and this widened to 14 percentage points at release testing, with controls remaining behind experimentals. None of the final differences between experimentals and controls yielded a statistically significant finding.

Comparisons between contract and non-contract experimentals indicated rather minor differences at the time of intake, with those who were later to secure and complete contracts holding a slight initial advantage. Between intake and release, the non-contract experimental sample showed essentially no increase in the proportion of members claiming that the entirety, or at least half the preparation for their intended post-release job was obtained during the prison stay, and the proportion claiming no relevant prison preparation actually climbed substantially -- from 58% at the time of intake to 71% at release. This latter finding is possibly attributable to plans spoiled during the period between tests, or to disillusionment. Contract experimentals, over this same period, showed substantial increase in the proportion claiming job-relevant prison preparation, to establish statistically significant final advantage over those without contract.

TABLE 6

PRISON PREPARATION FOR JOB ANTICIPATED AT RELEASE
RESPONSES ON INTAKE AND RELEASE QUESTIONNAIRES -- ARIZONA

GROUP	"NONE" INTAKE	"NONE" RELEASE	"NONE" DIFF	"HALF OR MORE" INTAKE	"HALF OR MORE" RELEASE	"HALF OR MORE" DIFF	"ALL" INTAKE	"ALL" RELEASE	"ALL" DIFF
STUDY SAMPLE	62%	52%	-10%	21%	38%	+17%	14%	24%	+10%
CONTROL	69	62	- 7	16	29	-13	12	14	+ 2
	≻(10%)	≻(14%)		≻(8%)	≻(13%)		≻(3%)	≻(14%)	
EXPERIMENTAL	59	48	-11	24	42	+18	15	28	+13
CONTRACT	59	38	-21	28	54	+26	16	35	+19
	≻(1%)	≻(33%)		≻(9%)	≻(36%)		≻(4%)	≻(20%)	
NON-CONTRACT	58	71	+13	19	18	- 1	12	15	+ 3

INSTITUTION STAFF OPINIONS OF THE MUTUAL AGREEMENT PROGRAM

Some nine months after inception of the MAP project at the Fox Lake institution in Wisconsin, a survey was conducted to solicit impressions from institution staff about the program and its participants. No staff survey was ever carried out in Arizona.

A 26 item true-false instrument (see next page) was administered, and protocols were returned by 66 staff members. Four subjects who had failed to commit themselves to a true or false answer on more than 20 items were dropped from further consideration; of the 62 remaining, 19 identified themselves as work supervisors, 13 as teachers, 9 as vocational instructors, 7 as counselors or therapists, and 6 as officers. Eight other respondents from various occupational specialties were placed in a "miscellaneous" category.

Overall, the response distribution for the 62 member study sample was: 31% "true," 65% "false," and 4% non-response or non-commital response. On thirteen items phrased in such a way that a "true" response was favorable to the program, 40% of the obtained responses were "true." On thirteen items structured to provide that a "true" response was unfavorable to the program, 22% of obtained responses were "true." Some differences in general sentiment toward the program were evident by occupational category, with about half the responses of officers, counselors and therapists being true to favorable worded items, compared to two-fifths for work supervisors and vocational instructors, and one-third for teachers.

Respondents were asked to indicate how many MAP cases they had worded with, and a comparison was made to determine whether those having contact with more MAP cases differed in sentiment from those with less contact. For the twenty staff members who had worked with seven or more MAP cases, the true response rate to favorably worded items was 40%; for 22 staff members who had worked with four to six cases, the rate was 45%; for 20 staff members who had worked with three of fewer cases, the rate was 33%. No simple and direct relationship appeared to exist between familiarity with MAP cases and sentiment toward the project.

Every questionnaire item was worded as an assertion which began: "In general, MAP cases..." Eighty-one percent of staff respondents expressed a belief that MAP cases were getting earlier release dates than they would otherwise have, and only 3% indicated that MAP cases ran substantial risk of staying in prison longer. Only twenty-four percent thought MAP cases got significantly better program opportunities and, on a similar item, 79% indicated that the cases had programs hardly different than before they got contracts. Despite the absence of belief in substantially improved program opportunity,

TABLE 7

MUTUAL AGREEMENT PROGRAM INSTITUTION STAFF QUESTIONNAIRE *

INSTRUCTIONS: The "MAP" demonstration project for contractual agreement between prisoner, parole board, and institution staff has been in operation for several months. We would appreciate your impressions and opinions about the project, and your judgment about what, if any, effects it is having upon prisoners and the operation of the institution.

A. RESPONDENT (check one)
_____Teacher _____Vocational Instructor _____Counselor or Therapist
_____Work Supervisor _____Other: ________________________________
B. I have worked with about _____ MAP cases (prisoners with contracts).
C. I am acquainted with about _____ MAP cases (prisoners with contracts).

IN GENERAL:

1. MAP cases take up too much of my time. (8%)
2. MAP cases are easier to work with. (37%)
3. MAP cases accomplish more than other prisoners. (36%)
4. MAP cases show less respect toward staff. (14%)
5. MAP cases benefit at the expense of other prisoners. (14%)
6. MAP cases are held to higher standards than other prisoners. (19%)
7. MAP cases hinder the orderly operation of the institution. (8%)
8. MAP cases are better mainly because only the better prisoners get contracts. (13%)
9. MAP cases get significantly better program opportunities. (24%)
10. MAP cases are a good influence on other prisoners. (42%)
11. MAP cases adjust better than before they were given contracts. (47%)
12. MAP cases are getting earlier release dates than they would otherwise have. (81%)
13. MAP cases make my own efforts more productive. (39%)
14. MAP cases conscientiously live up to their part of the bargain. (39%)
15. MAP cases are treated less firmly by institution staff. (29%)
16. MAP cases have programs hardly different than before they got contracts. (79%)
17. MAP cases are resented by other prisoners who have no contracts. (34%)
18. MAP cases are more enthusiastic about program involvement. (48%)
19. MAP cases do harder time than other prisoners. (11%)

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*"True" response percent shown in parentheses.

20. MAP cases will be less likely to recidivate because of the contract experience. (18%)

21. MAP cases run substantial risk of staying in prison longer. (3%)

22. MAP cases were promised more than can be provided. (19%)

23. MAP cases are disappointed with the program. (14%)

24. MAP cases are more interested in their rights than their responsibilities. (44%)

25. MAP cases obtain more insight into the nature of their problems. (31%)

26. MAP cases earn respect through their behavior and attitude. (40%)

PLEASE MAKE ANY COMMENTS YOU HAVE ON THE BACK OF THIS SHEET.

relatively few personnel believed that the MAP subjects were promised more than could be provided (19%) or that they were disappointed with the program (14%).

A somewhat greater tendency existed to believe that MAP cases were treated less firmly by institution staff (29% of respondents expressed such a belief) than to see MAP cases as being held to higher standards than other prisoners (19%). Only eleven percent of staff believed that MAP cases did harder time than other prisoners. While MAP subjects were not viewed as being placed under particular pressure, forty-seven percent of staff respondents suggested that the MAP cases adjusted better than before they were given contracts, and thirty-six percent believed they accomplished more than other prisoners. About half the staff (48%) found MAP subjects more enthusiastic about program involvement, and two-fifths (37% -- 40%) said these subjects were easier to work with, conscientiously lived up to their part of the bargain, earned respect through their behavior and attitude, and made the staff's own efforts more productive. (Although two-fifths of staff endorsed these last four items on program performance, it is necessary to keep in mind that three-fifths did not.)

Nearly half the respondents (44%) believed MAP cases to be more interested in their rights than their responsibilities, but only 14% claimed these cases showed less respect toward staff, and only 8% thought they hindered the orderly operation of the institution or took up too much of the respondents' own time. Though 42% found MAP cases a good influence on other prisoners, 34% also saw MAP cases as resented by other prisoners who had no contracts, and 14% thought project cases benefit at the expense of other prisoners. A small minority of staff (13%) believed MAP got contracts. In terms of deeper or longer range impact of participation in MAP, about a third of staff (31%) thought MAP subjects obtained more insight into the nature of their problems, and about a fifth (18%) expected MAP cases would be less likely to recidivate because of the contract experience.

On a number of items, particular occupation categories of institutional staff departed from the general level of opinion. Instances in which a given category of respondents departed from the norm by 20 percentage points or more were noted. Counselors and therapists were more likely than other staff, by this criterion, to complain that MAP cases took up too much of their time, but also believed these cases were held to higher standards, did harder time, were more enthusiastic, adjusted better, were a good influence on other prisoners, made their (staff) efforts more productive, and were less likely to recidivate. Officers were more likely than other staff to see MAP cases as easier to work with, accomplishing more, and earning respect even though contracts hadn't affected their programs, and less likely than other staff to see MAP cases as treated less firmly by staff or more interested in rights than responsibilities. Work supervisors were more inclined than other staff to find MAP cases showing less respect toward staff, while teachers were less likely than other staff to believe that MAP cases obtained greater insight into the nature of their problems, and vocations instructors were less inclined to think other prisoners resented MAP cases.

In general, these findings, based upon a reasonable amount of exposure to the MAP program and its participants, seem to represent a moderate acceptance by institution staff and a cautiously optimistic outlook for the program, tempered by some realistic qualifications and reservations. Complaints about effort and concerns about disruption were not prominent, but neither was faith in improved programming or effects on recidivism. A sizable proportion of staff seemed to endorse MAP as making a difference in program performance and prison adjustment, and most were convinced that the project was accomplishing shortened periods of institutional stay. If we were to base our interpretation of institution staff's responses to the MAP project simply on the basis of the central tendency for responses however, we face some danger of being misled. The distribution of respondents on the thirteen favorably worded items regarding the project was positively skewed: two respondents endorsed none of these items, nine respondents only one, and thirteen respondents only two, as compared to three willing to endorse eleven, two endorsing twelve, and none endorsing all thirteen. For the twenty-four staff members (about two-fifths of the total study sample) who endorsed two or fewer favorable worded items, a total of 35 endorsements were received, about half of which were concessions that MAP cases got earlier release dates. These same respondents endorsed 80 unfavorably phrased items, with the most frequent complaints being that programs were hardly different than before contract, that MAP subjects were more interested in rights than responsibilities, that they were treated less firmly by staff and were resented by prisoners without contracts. A detailed commentary was prepared by one of these critics, and may be found in the Appendix to this report. It is included not as representative of prison staff opinion, but as an example of the kinds of misgivings that arose around the project. MAP had supporters and detractors among institution staff; the majority included some who accepted it as beneficial, and others who were willing to tolerate it while remaining skeptical about its benefits. A few were sufficiently hostile toward the project to be likely to make it a subject of continuing controversy.

(There was, unfortunately, no survey of staff impressions carried out in the other two states, but ARizona, the parole board included, in its annual Report, a statement about its perspective of the experience with contract programming. See Appendix.)

COMPARISONS AT RELEASE

SUMMARY

EXPERIMENTAL VS. CONTROL

Given the numerous variables compared at the time of release, about 5 statistically significant at $p<.10$ and two at $p<.05$ might be expected to arise by chance. Since these variables are occasionally quite similar in content, a significant finding on two could be expected to accompany such a finding on another. Nine comparisons actually yielded difference significant at least at the trend threshold, and, among these nine, five reached $p<.05$, three $p<.025$, two $p<.005$, and one $p<.001$.

Arizona experimentals were notably more likely to be released from prison earlier than controls, as measured in months from project intake. Nine months subsequent to project intake, a 34% difference existed ($p<.001$), and later, at the close of data collection, experimentals still held an advantage, with 16% more cases released than controls ($p<.01$). Even so, it was found that Arizona experimentals had actually served, on the average, about three more months between prison admission and release than members of the control sample, because they tended to have entered prison earlier. Use of median rather than mean as the index for comparison, indicates equal periods -- 24 months -- served in prison by experimentals and controls. Because of the initial difference in Arizona, it is not possible to make any definitive separation of the effects of the "head start" and the consequences of contract involvement.

In Wisconsin, months served in prison prior to project intake, and months served subsequent to intake were approximately the same for experimentals and controls.

At trend significance ($p<.10$), Arizona experimentals were about 20 percentage points more likely than controls to claim belief that the prison programs in which they had been involved would help them, by at least "a fair amount," to obtain a decent job after release. Similarly, they were more likely, by 20 percentage points to claim they had obtained more than six months coursework in prison for the job they expected to find after release ($p<.10$).

In Wisconsin, experimentals were 12 percentage points less likely than controls to claim prison work experience in excess of six months for the job they intended to obtain when released. Experimentals were, however, consistently more likely to have faith that MAP programming had helped them in several specified areas than for controls to believe that a contract would have helped them. The most marked differences concerned MAP's benefit relative to release date certainty ($p<.001$); to facilitating arrangements in the community -- 19 percentage points -- ($p<.05$), to easing the passage of prison time -- 18 percentage points -- ($p<.05$), and to stimulate interest in working harder -- 15 percentage points -- ($p<.10$).

CONTRACT VS. NON-CONTRACT

Relative to Wisconsin, control subjects in Arizona were considerably less skeptical about the benefits of MAP programming; differences were, however, frequently large between contract and non-contract experimentals, with both potential MAP benefits foreclosed, and about assistance from the prison programs they had received. Thus, belief that programs received would help them obtain a decent job and claims of prison work experience relevant to the job they sought were notably higher for contract than non-contract experimentals in Arizona (differences of 36 percentage points ($p<.005$), and of 27 percentage points ($p<.025$), respectively). Similarly, differences existed for belief in MAP assistance relative to eliciting staff interest in helping -- 29 percentage points ($p<.01$), stimulating one's own interest in working harder -- 27 percentage points ($p<.025$), and for MAP's capacity to help them get a job -- 20 percentage points ($p<.01$) and to stay out of prison in the future -- 18 percentage points ($p<.10$). Contract experimentals were also far more likely to anticipate taking, after release, a type of job they had never held before ($p<.025$). To a moderate extent, such differences are a product of the Arizona parole board's greater reluctance to accept into contracts prisoners who did not already have a relatively high amount of training already completed or nearing completion, so caution is warranted in attributing reported differences to effects occurring after contract entry.

In both Arizona and Wisconsin, experimentals under contract were markedly (46 percentage points and 22 percentage points, $p<.001$, $p<.025$) more likely to attain release within nine months subsequent to project intake, and the difference is obviously largely attributable to selective screening for contract entry -- reasons for contract denial were frequently stated in terms of the board's unwillingness to grant an early termination date because of either the gravity of commitment offense or seriousness of prior record (i.e., not "enough" time, given the project deadline for completion of all contracts granted). In both states, decliners of contract were substantially more likely than those denied or dropped to attain release within nine months -- 61 percentage points in Arizona ($p<.001$), 27 percentage points in Wisconsin ($p<.025$).

In both states, as well, contract experimental cases were substantially more likely -- 38 percentage points in Arizona ($p<.001$), and 24 percentage points in Wisconsin ($p<.025$) -- than non-contract experimentals to claim having received some prison coursework relevant to the job they hoped to obtain after release, and also more likely (by 35 percentage points in Arizona and by 32 percentage points in Wisconsin -- both significant at $p<.005$) to agree that a cause of their prison commitment offense had been low income or lack of work. Again, this difference appears primarily attributable to self-selection and screening effects at the point of contract entry, rather than to subsequent changes in perspective, since overall experimental vs. control differences in each state on this measure are slight.

STATUS FOLLOWING RELEASE

AND

DIFFERENCES AMONG COMPARISON GROUPS

Follow-up instruments were to be prepared by parole agents after one, three, and six month exposure periods in the community, or by any alternate arrangement, this failing, that state project coordinators could devise. In the two states where the original research design survived relatively intact -- Wisconsin and Arizona, we were fortunate to have project coordinators who were as diligently attentive to data collection as they were to operational matters, and essentially complete data sets are available for the institutional period. In Wisconsin, excellent control over data was maintained, as well, throughout the post-release follow-up period, with over 95% of the documents sought being acquired. In Arizona, a combination of circumstances made the collection of follow-up data more difficult and less successful, and increases the ambiguity of available findings. The problems included first, termination of jurisdiction (i.e., discharge) at the point of prison release, rather than parole supervision, early terminations for some released on parole, and transfer to states other than Arizona for thd period of parole supervision. These problems were compounded when, at the close of the project operating period, but several months before close of data collection, the state coordinator moved and the parties who had agreed to assume his data monitoring responsibility failed in their commitment. Even so, assisted by examination of case files, it was possible to salvage basic measures for over 80% of the cases on which follow-up information was due.

Follow-up was conducted through the end of June, 1974 in Wisconsin. By that cut-off date, nearly 90% of the full 200 member study sample was due for 1 month follow-up, 80% for 3 months, and 75% for 6 months. For each of these comparison periods, nearly identical "shares" of the control and experimental groups fell due for inspection. In Arizona, by the end-of-May, 1974 cut-off date, slightly over 80% of the 196 member study sample was due for 1 month follow-up, slightly under 80% for 3 months, and 72% for 6 months. Because of differential release rates among comparison groups, however, the "share" of experimentals and controls due for inspection differs, as well as does the thoroughness of information supply on those due for inspection.

For all comparisons, percents have been computed using data available, rather than data due as the base, or denominator. In Wisconsin, the choice of base in inconsequential, the comparisons fairly straightforward. Because of the greater murkiness in Arizona data, and the increase difficulty in interpreting its meaning, conclusions from the available findings are, necessarily, more tentative. The "Data Limits" tables which follow, show the extent of data loss for each comparison group in each state, and for each follow-up period.

WISCONSIN

Post-release Follow-up

TABLE 8

Data Limits

	NON-CONTRACT		CONTRACT		FULL EXPERIMENTAL		CONTROL		FULL STUDY SAMPLE		X—C DIFF
	f	% of Total	f	% of Total	f	% of Total	f	% of Total	f	% of Total	
SAMPLE SIZE	82		68		150		50		200		
DATA DUE											
1 MO. FOLLOW-UP	62	76%	68	100%	130	87%	44	88%	174	87%	1%
3 MO. FOLLOW-UP	53	65%	68	100%	121	81%	39	78%	160	80%	3%
6 MO. FOLLOW-UP	47	57%	68	100%	115	77%	35	70%	150	75%	7%
	f	% of Due	f	% of Due	f	% of Due	f	% of Due	f	% of Due	
DATA AVAILABLE											
AT 1 MONTH	58	94%	68	100%	126	97%	41	93%	167	96%	4%
AT 3 MONTHS	51	96%	68	100%	119	98%	38	97%	157	98%	1%
AT 6 MONTHS	43	92%	68	100%	111	96%	32	91%	143	95%	5%

ARIZONA

Post-release Follow-up

Data Limits

	NON-CONTRACT		CONTRACT		FULL EXPERIMENTAL		CONTROL		FULL STUDY SAMPLE		X—C DIFF
	f	% of Total	f	% of Total	f	% of Total	f	% of Total	f	% of Total	
SAMPLE SIZE	55		75		130		65		195		
DATA DUE											
1 MO. FOLLOW-UP	38	69%	74*	99%	112	86%	45	69%	157	81%	17%
3 MO. FOLLOW-UP	37	67%	74	99%	111	85%	41	63%	152	78%	22%
6 MO. FOLLOW-UP	34	52%	72**	96%	106	82%	34	52%	140	72%	30%
	f	% of Due	f	% of Due	f	% of Due	f	% of Due	f	% of Due	
DATA AVAILABLE											
AT 1 MONTH	25	66%	74	100%	99	88%	31	69%	130	83%	19%
AT 3 MONTHS	25	68%	72	97%	97	87%	27	66%	124	82%	21%
AT 6 MONTHS	22	65%	69	96%	91	86%	24	71%	115	82%	15%

* 1 case released to detainer.

** 1 deceased; 1 not released

CRIMINAL JUSTICE SYSTEM DISPOSITIONS AT ONE MONTH

WISCONSIN AND ARIZONA

A cohort follow-up procedure was employed, with exposure to risk beginning with release from prison to either parole or discharge, and incidents classified according to Uniform Parole Report definitions for measuring "Most Serious Disposition."

Among the members of the study sample on which outcomes were due and available for the one month follow-up period, no case in either Wisconsin or Arizona had yet passed the UPR threshold for "Unfavorable Outcome" (return to prison or suspended prison sentence, absconder with felony warrant, death by crime or overdose, jail sentence of 90 or more days, or five or more years probation).

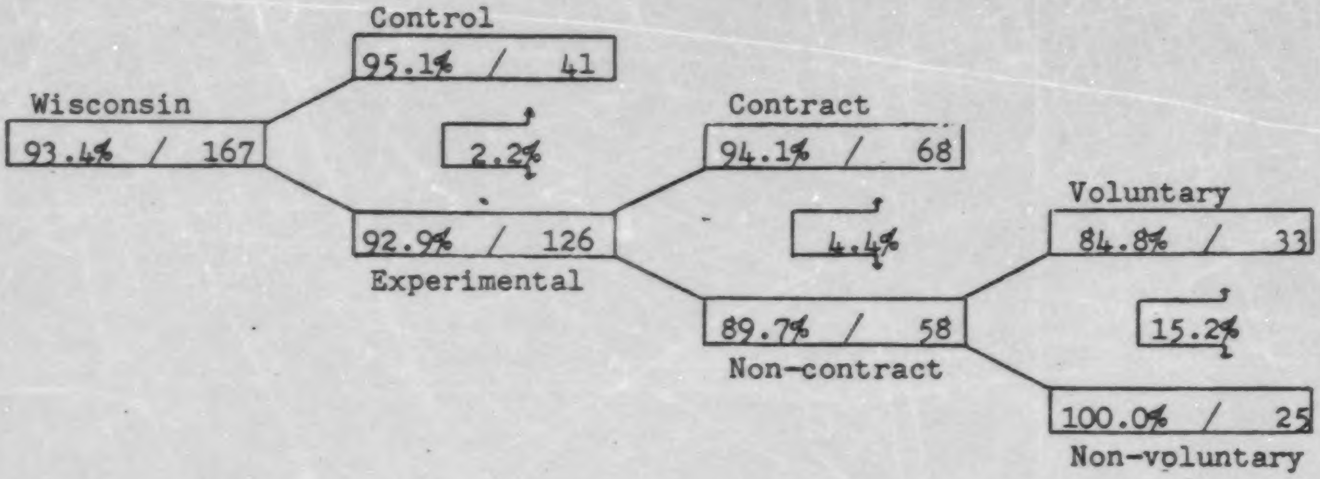
In Wisconsin, 93% of subjects experienced no official difficulty during the first month following release, and in Arizona, 95% experienced no difficulty. The remainder either had dispositions classified as minor (convicted and sentenced to jail term of under 90 days or to any amount of suspended jail time, fine, bail forfeiture or probation less than five years, absconder without felony warrant, arrested and released), or dispositions which remained pending and without final resolution (either outcomes of trial or sentencing, or of parole board decision) throughout the entire period of data collection, or throughout the period for which it was possible to trace a given subject.

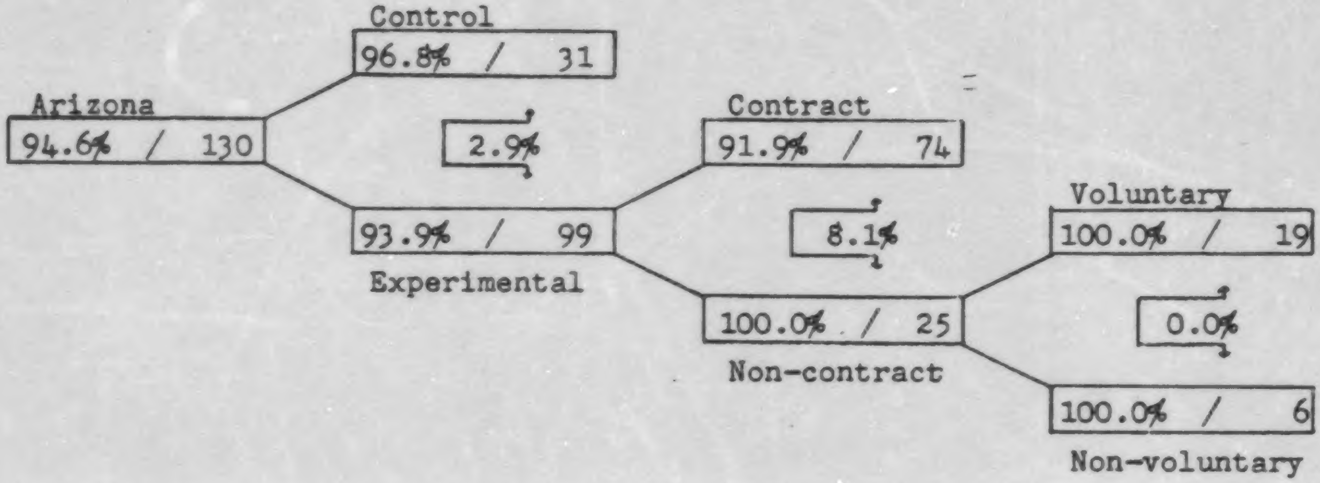
Among comparison groups in Wisconsin 95% of controls and 93% of experimentals remained "clean" or arrest-free during one month follow-up. In Arizona, 97% of controls and 94% of experimentals were similarly clean.

Within the experimental sample, Wisconsin cases under contract were slightly more likely (4 percentage points) to be arrest-free than those without contract; all non-voluntary drops remained arrest-free, but 15% of voluntary drops had already received a minor disposition or were in pending status by the close of their first month after release. In Arizona, 100% of non-contract experimentals were arrest-free at one month, compared to 92% of contract cases. To some extent the difference may be attributed to more thorough availability of information upon cases under contract.

37 POST-RELEASE

ARREST-FREE AT ONE MONTH SUBSEQUENT TO RELEASE


Wisconsin
93.4% / 167
Control
95.1% / 41
2.2%
92.9% / 126
Experimental
Contract
94.1% / 68
4.4%
89.7% / 58
Non-contract
Voluntary
84.8% / 33
15.2%
100.0% / 25
Non-voluntary


Arizona
94.6% / 130
Control
96.8% / 31
2.9%
93.9% / 99
Experimental
Contract
91.9% / 74
8.1%
100.0% / 25
Non-contract
Voluntary
100.0% / 19
0.0%
100.0% / 6
Non-voluntary

CRIMINAL JUSTICE SYSTEM DISPOSITIONS AT THREE MONTHS

WISCONSIN AND ARIZONA

Only two Wisconsin cases (both of them non-contract experimentals) and one Arizona case (a contract experimental) were returned to prison by the end of the third month following release, and no other major dispositions were accorded during that follow-up period.

Increases in minor and in pending disposition categories occurred, leaving 82% of the Wisconsin follow-up sample and 78% of the Arizona sample still without official difficulty by the end of the third month of exposure to risk. In both Wisconsin and Arizona, controls continued to outperform experimentals by a slight margin in terms of the proportion who remained arrest-free. In Arizona, the advantage of non-contract experimentals over those with contract had grown to 19 percentage points, a difference significant at trend level ($X^2 = 3.65$; $p < .10$), but one which might be accounted for by several alternate explanations, including:

a. Statistical artifact arising on the basis of chance whenever numerous comparisons are made;

b. Differences in original risk level introduced through the screening and selection phase of contract entry;

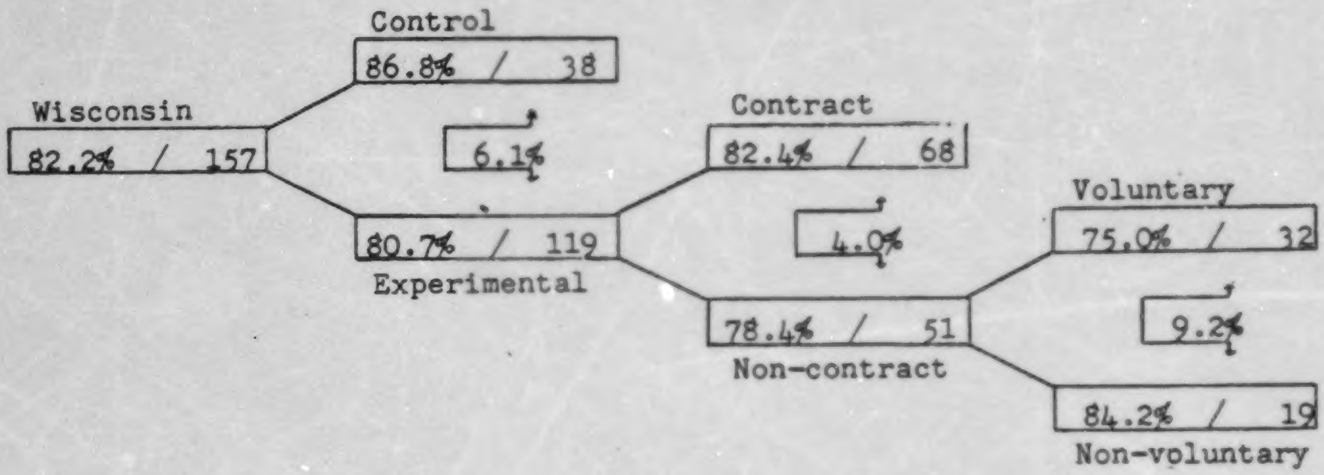
c. Differences in representativeness of members of the exposed subsamples (those on whom information was due) and the available subsamples (those on whom information was obtained);

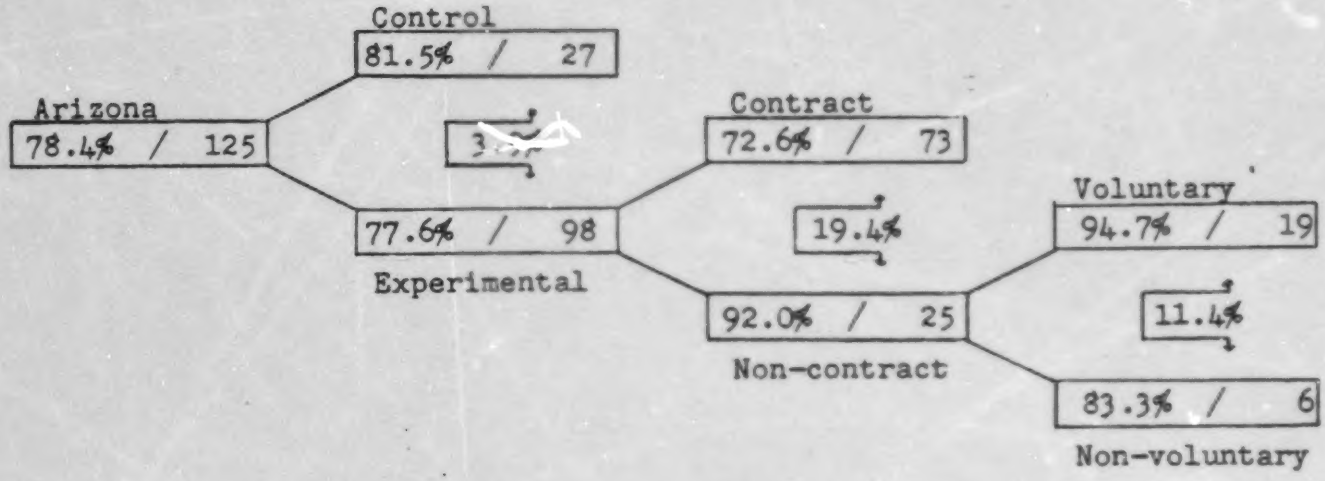
d. Differences in surveillance intensity because of differing proportions paroled or discharged;

e. Negative effects of contract programming.

Given the uncertainties involved, and the small sample size available, the first of the above listed possibilities is most parsimonious and at least as tenable as the others. The list does, however, serve to illustrate the types of ambiguity to which the Arizona follow-up findings are subject.

38 POST-RELEASE
ARREST-FREE AT THREE MONTHS SUBSEQUENT TO RELEASE


Wisconsin
82.2% / 157
Control
86.8% / 38
6.1%
80.7% / 119
Experimental
Contract
82.4% / 68
4.0%
78.4% / 51
Non-contract
Voluntary
75.0% / 32
9.2%
84.2% / 19
Non-voluntary


Arizona
78.4% / 125
Control
81.5% / 27
3.[illegible]
77.6% / 98
Experimental
Contract
72.6% / 73
19.4%
92.0% / 25
Non-contract
Voluntary
94.7% / 19
11.4%
83.3% / 6
Non-voluntary

CRIMINAL JUSTICE SYSTEM DISPOSITIONS AT SIX MONTHS

WISCONSIN AND ARIZONA

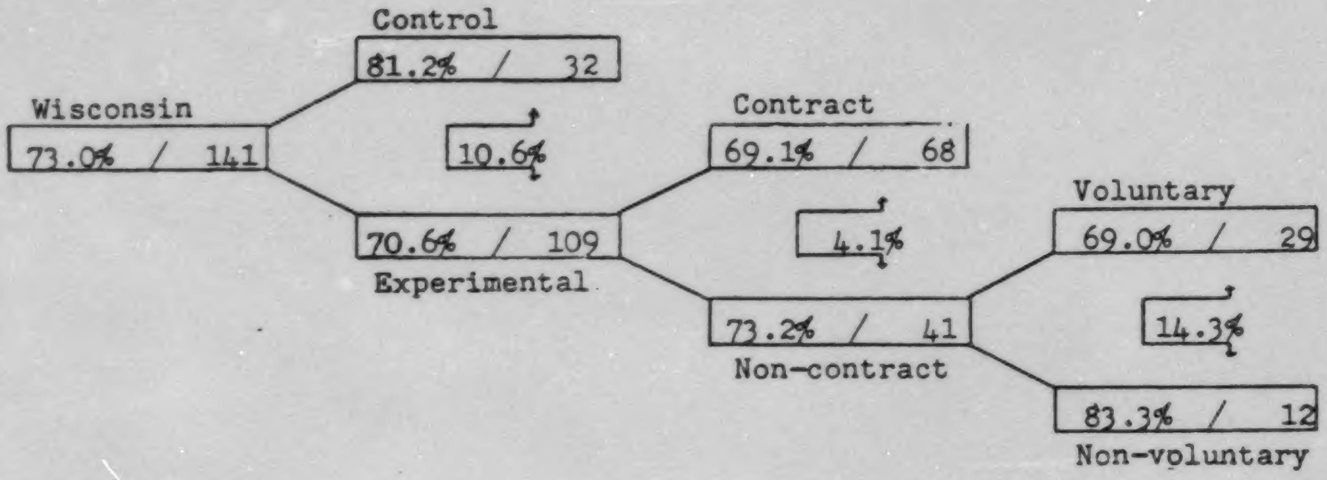
By the end of six months subsequent to release, no control case in either Wisconsin or Arizona was reported to have yet been returned to prison, and only one (in Arizona) had suffered any other form of major disposition. The pro return rate for experimentals was 4% in Wisconsin, and 8% in Arizona; for experimentals with contract, the rates were 2% (Wisconsin) and 7% (Arizona).

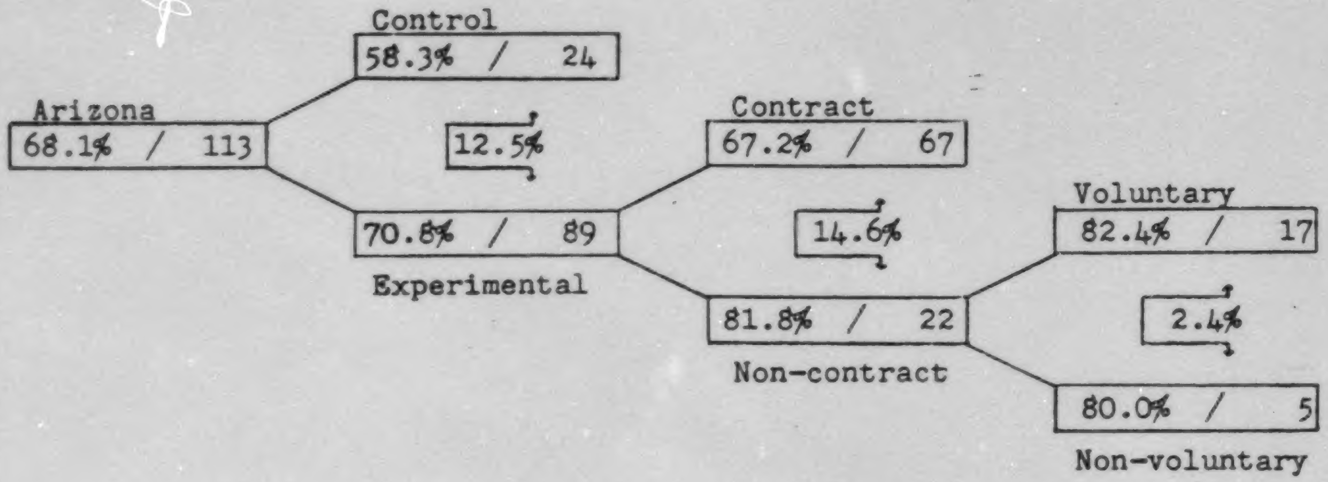
By the arrest-free criterion 73% of Wisconsin cases and 68% of Arizona cases remained "clean" throughout the six month follow-up period. In Wisconsin, controls held an 11 percentage point edge over experimentals while, in Arizona, experimentals held a 12 percentage point edge over controls; neither finding is statistically significant at even trend level, but compared with the results for the three month follow-up period, controls increased their advantage slightly over experimentals in Wisconsin, while Arizona experimentals succeeded in overcoming their earlier disadvantage.

Within the Wisconsin experimental sample, non-contract moved from slightly behind those with contract in the early follow-up periods to a position slightly ahead of them by the end of six months follow-up; non-voluntary drops from contract performed consistently better than voluntary drops at each exposure period.

In the Arizona experimental sample, non-contract cases continued to hold a 15 percentage point lead over cases with contract, but the difference no longer reached trend significance level, partly owing to the shrinkage in sample size with sufficient exposure and available data.

39 POST-RELEASE
ARREST-FREE AT SIX MONTHS SUBSEQUENT TO RELEASE


Wisconsin
73.0% / 141
Control
81.2% / 32
10.6%
Experimental
70.6% / 109
Contract
69.1% / 68
4.1%
Non-contract
73.2% / 41
Voluntary
69.0% / 29
14.3%
Non-voluntary
83.3% / 12


Arizona
68.1% / 113
Control
58.3% / 24
12.5%
Experimental
70.8% / 89
Contract
67.2% / 67
14.6%
Non-contract
81.8% / 22
Voluntary
82.4% / 17
2.4%
Non-voluntary
80.0% / 5

WISCONSIN

TABLE 10

Post-release Follow-up

Criminal Justice System Dispositions

VIOLATION THRESHOLD	NON-CONTRACT		CONTRACT		FULL EXPERIMENTAL		CONTROL		FULL STUDY SAMPLE		X--C DIFF
	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	
AT 1 MONTH											
ARREST-FREE*	53	91%	64	94%	117	93%	39	95%	156	93%	- 2%
PENDING DISPO.	5	9%	4	6%	9	7%	2	5%	11	7%	- 2%
MINOR DISPO.	2	3%	1	2%	3	2%	1	2%	4	2%	0%
MAJOR DISPO.	0	0%	0	0%	0	0%	0	0%	0	0%	0%
PRISON DISPO.	0	0%	0	0%	0	0%	0	0%	0	0%	0%
AT 3 MONTHS											
ARREST-FREE*	40	78%	56	82%	96	81%	33	87%	129	82%	- 6%
PENDING DISPO.	11	22%	12	18%	23	19%	5	13%	28	18%	- 6%
MINOR DISPO.	8	16%	8	12%	16	13%	3	8%	19	12%	- 5%
MAJOR DISPO.	2	4%	0	0%	2	2%	0	0%	2	1%	- 2%
PRISON DISPO.	2	4%	0	0%	2	2%	0	0%	2	1%	- 2%
AT 6 MONTHS											
ARREST-FREE*	30	70%	47	69%	77	69%	26	81%	103	72%	-12%
PENDING DISPO.	13	30%	21	31%	34	31%	6	19%	40	28%	-12%
MINOR DISPO.	10	23%	15	22%	25	22%	4	12%	29	20%	-10%
MAJOR DISPO.	3	7%	1	2%	4	4%	0	0%	4	3%	- 4%
PRISON DISPO.	3	7%	1	2%	4	4%	0	0%	4	3%	- 4%

* Arrest-free f and % are excluded from cumulative.

ARIZONA

TABLE 11

Post-release Follow-up

Criminal Justice System Dispositions

VIOLATION THRESHOLD	NON-CONTRACT		CONTRACT		FULL EXPERIMENTAL		CONTROL		FULL STUDY SAMPLE		X—C DIFF
	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	CUM f	% of AVAIL	
AT 1 MONTH											
ARREST-FREE*	25	100%	68	92%	93	94%	30	97%	123	95%	- 3%
PENDING DISPO.	0	0%	6	8%	6	6%	1	3%	3	7%	- 3%
MINOR DISPO.	0	0%	2	3%	2	2%	0	0%	2	2%	- 2%
MAJOR DISPO.	0	0%	0	0%	0	0%	0	0%	0	0%	0%
PRISON DISPO.	0	0%	0	0%	0	0%	0	0%	0	0%	0%
AT 3 MONTHS											
ARREST-FREE*	23	92%	53	74%	76	78%	22	81%	98	79%	- 3%
PENDING DISPO.	2	8%	19	26%	21	22%	5	19%	26	21%	- 3%
MINOR DISPO.	2	8%	11	15%	13	13%	3	11%	16	13%	- 2%
MAJOR DISPO.	0	0%	1	1%	1	1%	0	0%	1	1%	- 1%
PRISON DISPO.	0	0%	1	1%	1	1%	0	0%	1	1%	- 1%
AT 6 MONTHS											
ARREST-FREE*	18	82%	45	65%	63	69%	14	58%	77	67%	+11%
PENDING DISPO.	4	18%	24	35%	28	31%	10	42%	38	33%	+11%
MINOR DISPO.	3	14%	13	19%	16	18%	4	17%	20	17%	- 1%
MAJOR DISPO.	2	9%	6	9%	8	9%	1	4%	9	8%	- 5%
PRISON DISPO.	2	9%	5	7%	7	8%	0	0%	7	6%	- 8%

* Arrest-free f and % are excluded from cumulative.

OFFENSE CHARGED ON MOST SERIOUS DISPOSITION WITHIN SIX MONTHS FOLLOWING RELEASE

WISCONSIN AND ARIZONA

In Wisconsin, the offense resulting in most serious disposition for members of the study sample experiencing difficulty was often at misdemeanor level, with these charges accounting for about two-fifths the persons in difficulty in both the experimental and control samples. In Arizona, the most frequent charge against both experimentals and controls was absconding, with accounted for about one-third of the persons in difficulty. These findings are for the six month follow-up period.

At the felony level, homicide charges were brought against only one subject in each state, and both the Wisconsin and Arizona cases were experimentals who had completed MAP agreements. No assault or sex offense charges occurred in Wisconsin; in Arizona, two control cases were charged with assault and three experimentals were charged with sexual offenses, including one for rape and one for perversion among those completing contracts, and one for perversion among the non-contract experimentals.

One control and two contract experimental cases were charged with robbery in Wisconsin; one control and three contract experimental cases in Arizona faced robbery charges Burglary was charged against four Wisconsin experimentals -- three of these non-contract cases; in Arizona, two controls and three experimentals -- all with contracts -- were charged with burglary. Charges of grand theft or fraud were brought against two contract experimentals and two non-contract experimentals in Wisconsin, and against one Arizona contract experimental. Forgery charges were faced by one contract experimental in each of the two states. Drug offenses were charged against one Arizona contract experimental, and against four Wisconsin experimentals -- three of them under contract. One Arizona experimental without contract was charged with escape.

TABLE 12

OFFENSE CHARGED IN MOST SERIOUS DISPOSITION
SIX MONTH FOLLOW-UP PERIOD
WISCONSIN AND ARIZONA

WISCONSIN					OFFENSE	ARIZONA				
SS	C	X	Ct	NC		SS	C	X	Ct	NC
1		1	1		Homicide	1		1	1	
					Assault	2	2			
					Sex	3		3	2	1
					Arson	1		1	1	
3	1	2	2		Robbery	4	1	3	3	
4		4	1	3	Burglary	5	2	3	3	
4		4	2	2	Grand Theft	1		1	1	
2	1	1	1		Forgery	1		1	1	
4		4	3	1	Drugs	1		1	1	
					Escape	1		1		1
15	3	12	9	3	Misdemeanors	4	1	3	2	1
3	2	1	1		Absconder	12	4	8	7	1
36	7	29	20	9	TOTAL	36	10	26	22	4

SS = Study Sample
C = Control
X = Experimental
Ct = Contract Experimental
NC = Non-contract Experimental

C + X = SS

Ct + NC = X

FAITH IN MAP SUBSEQUENT TO PRISON RELEASE

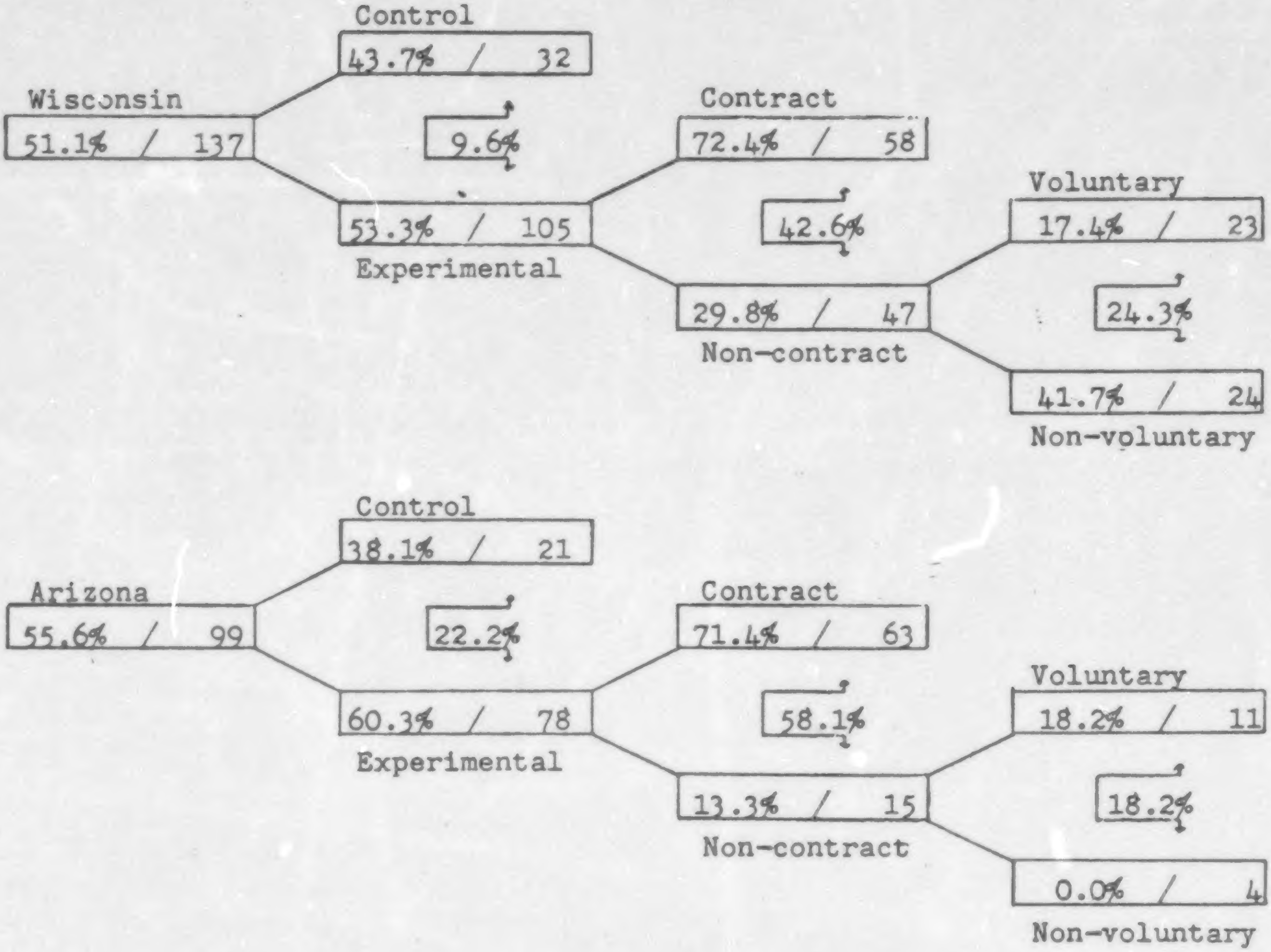
WISCONSIN AND ARIZONA

Members of the MAP study sample were to be asked, at the time of interview when other follow-up information was obtained, two questions about the value of assistance that MAP had provided them; one item concerned post-release employment, and the other asked if MAP had helped them "in any way at all."

About half the subjects in each state who responded to the item about help of any kind (51% in Wisconsin, 56% in Arizona) answered "yes" at the end of one month following release. At six months exposure, the level of MAP endorsement remained unchanged in Wisconsin, and fell by about 10 percentage points.in Arizona. In both states and for both exposure periods, experimentals who had completed contracts were markedly more likely than non-contract experimentals to agree that MAP had in some way assisted them, with two-thirds to three-fourths of the former respondents claiming help, and never more than a third of the latter. Surprisingly , about two-fifths of control subjects in both Wisconsin and Arizona who responded to the item one month following release claimed that MAP had helped them in some way; and the difference between controls and experimentals fails, in each state, to reach even trend level significance. Belief in personal assistance from MAP was more transient among controls than experimentals, however, and the six month follow-up comparison yields statistically significant differences in both states (Wisconsin: $X^2 = 6.76$; $p < .01$; Arizona: $X^2 = 6.97$; $p < .01$).

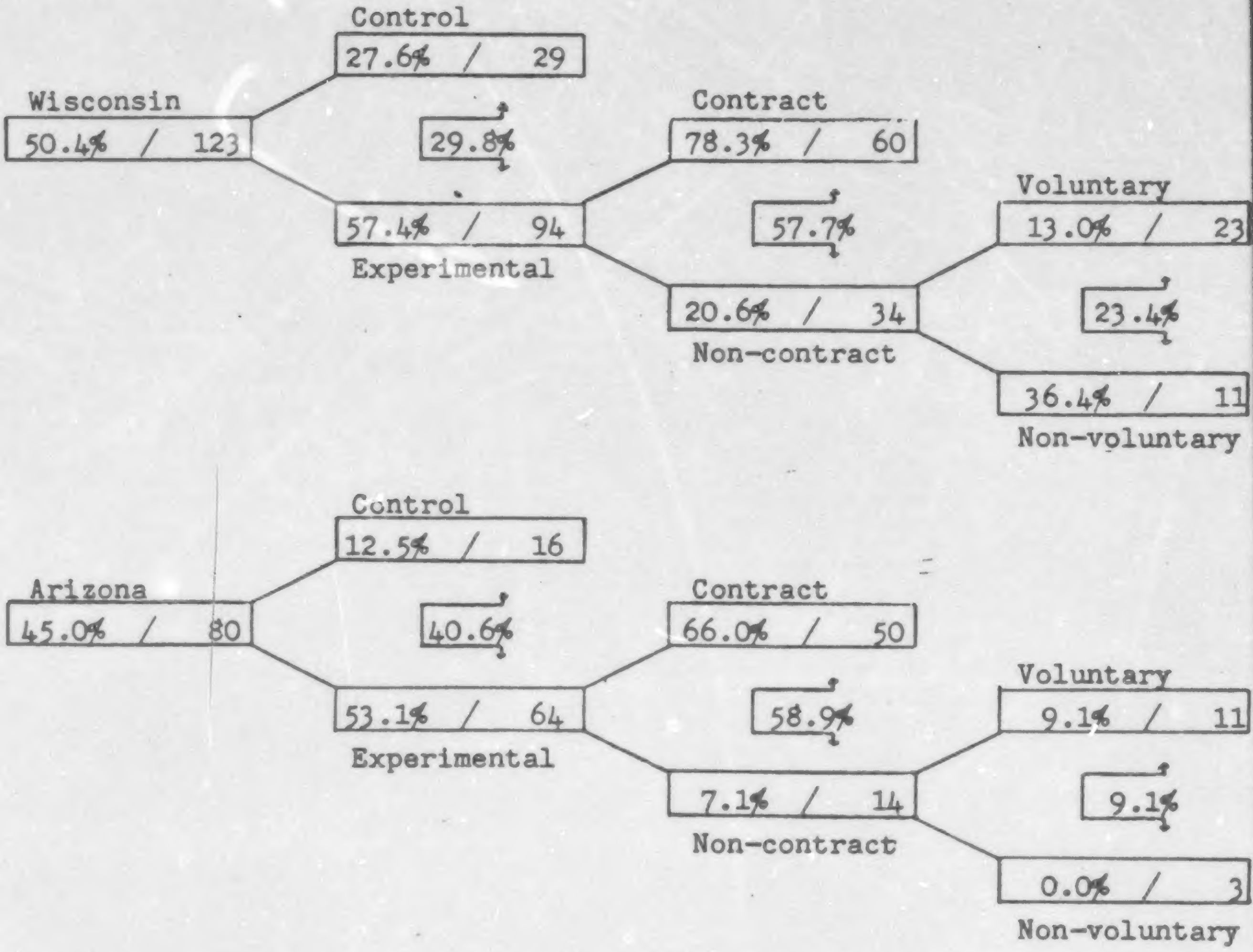
The pattern of endorsement, and of relative differences among comparison groups for the employment item, generally paralleled that for the item about any kind of help, although the overall level of endorsement and the magnitude of absolute percent differences tended to be less. Differences between experimentals and controls were greatest in Arizona, and differences between contract and non-contract experimentals greater in Wisconsin. No experimental vs. control differences were found to be statistically significant, but, for the sample sizes involved, quite large percentage differences are necessary to demonstrate association.

40 1ST MONTH POST-RELEASE
RELEASEE BELIEVED MAP HELPED HIM IN SOME WAY

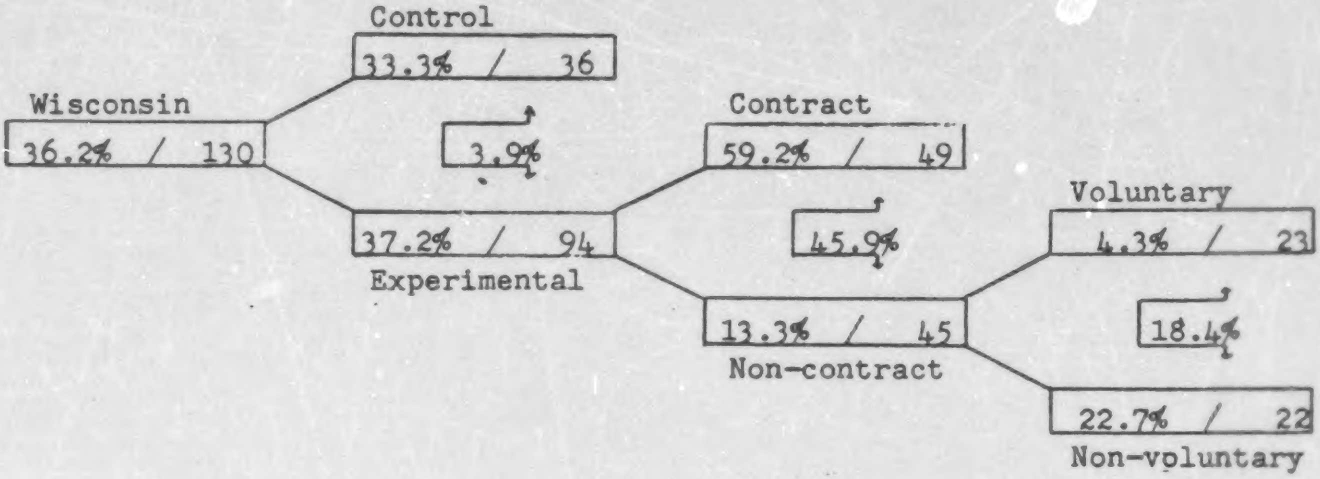


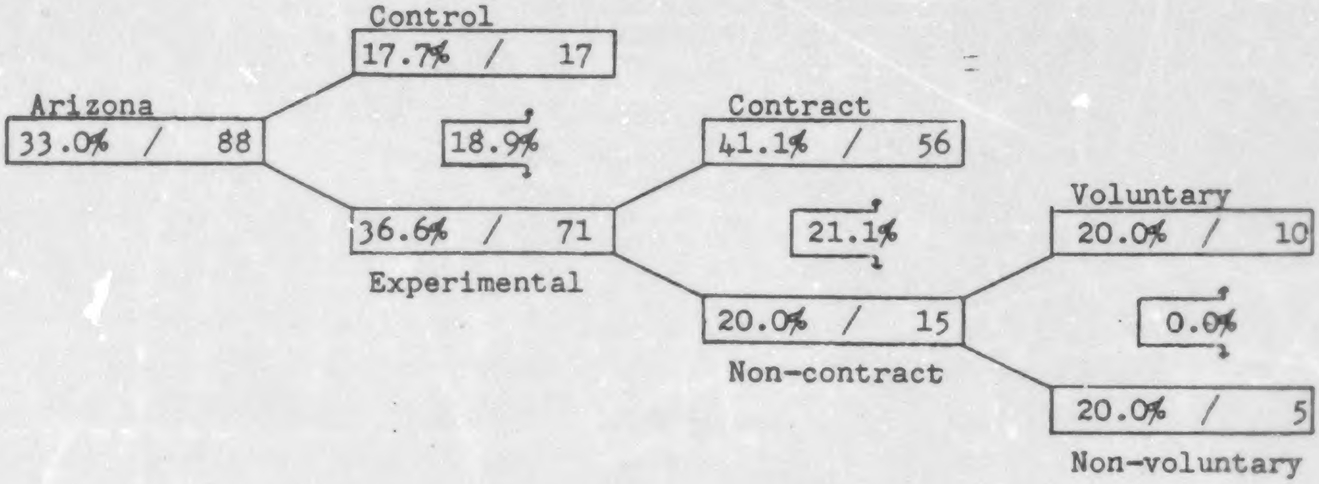
Wisconsin
51.1% / 137
Control
43.7% / 32
9.6%
53.3% / 105
Experimental
Contract
72.4% / 58
42.6%
29.8% / 47
Non-contract
Voluntary
17.4% / 23
24.3%
41.7% / 24
Non-voluntary
Arizona
55.6% / 99
Control
38.1% / 21
22.2%
60.3% / 78
Experimental
Contract
71.4% / 63
58.1%
13.3% / 15
Non-contract
Voluntary
18.2% / 11
18.2%
0.0% / 4
Non-voluntary

41 6TH MONTH POST-RELEASE
RELEASEE BELIEVED MAP HELPED HIM IN SOME WAY

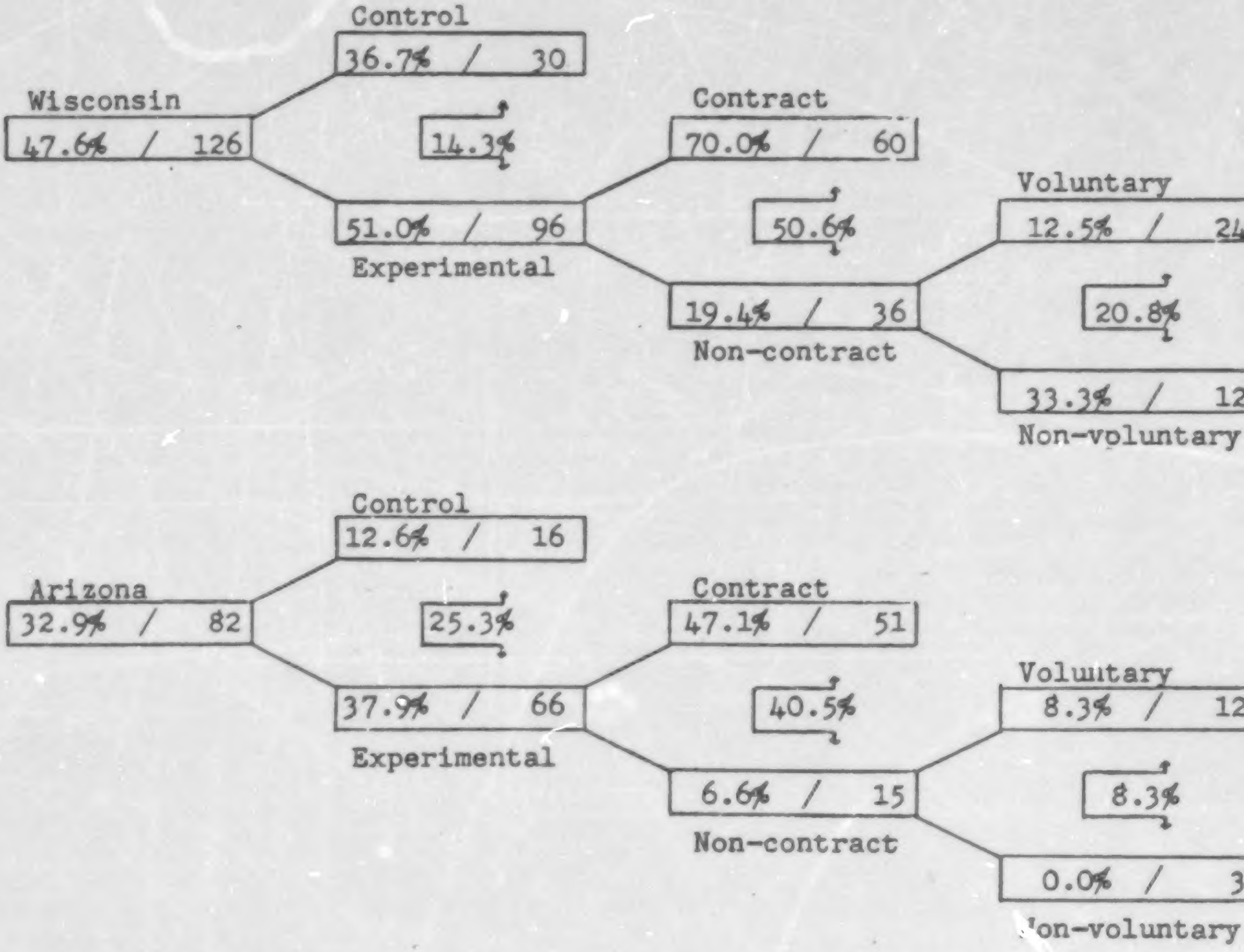

Wisconsin
50.4% / 123
Control
27.6% / 29
29.8%
57.4% / 94
Experimental
Contract
78.3% / 60
57.7%
20.6% / 34
Non-contract
Voluntary
13.0% / 23
23.4%
36.4% / 11
Non-voluntary
Arizona
45.0% / 80
Control
12.5% / 16
40.6%
53.1% / 64
Experimental
Contract
66.0% / 50
58.9%
7.1% / 14
Non-contract
Voluntary
9.1% / 11
9.1%
0.0% / 3
Non-voluntary

42 1ST MONTH POST-RELEASE
RELEASEE BELIEVED MAP HELPED AT LEAST A SMALL AMOUNT IN MAKING JOB EXPERIENCE AFTER RELEASE MORE PLEASANT OR PRODUCTIVE


Wisconsin
36.2% / 130
Control
33.3% / 36
3.9%
37.2% / 94
Experimental
Contract
59.2% / 49
45.9%
13.3% / 45
Non-contract
Voluntary
4.3% / 23
18.4%
22.7% / 22
Non-voluntary


Arizona
33.0% / 88
Control
17.7% / 17
18.9%
36.6% / 71
Experimental
Contract
41.1% / 56
21.1%
20.0% / 15
Non-contract
Voluntary
20.0% / 10
0.0%
20.0% / 5
Non-voluntary

43 6TH MONTH POST-RELEASE
RELEASEE BELIEVED MAP HELPED AT LEAST A SMALL AMOUNT IN MAKING JOB EXPERIENCE AFTER RELEASE MORE PLEASANT OR PRODUCTIVE



Control
36.7% / 30
Wisconsin
47.6% / 126
14.3%
Contract
70.0% / 60
51.0% / 96
Experimental
50.6%
Voluntary
12.5% / 2
19.4% / 36
Non-contract
20.8%
33.3% / 12
Non-voluntary
Control
12.6% / 16
Arizona
32.9% / 82
25.3%
Contract
47.1% / 51
37.9% / 66
Experimental
40.5%
Voluntary
8.3% / 12
6.6% / 15
Non-contract
8.3%
0.0% / 3
on-voluntary

ACCESS TO EMPLOYMENT IN MONTH FOLLOWING RELEASE

WISCONSIN

Fifty-nine percent of the prisoners released in Wisconsin for whom data were available had obtained and were holding full-time jobs at the end of their first month in the community. A slightly (4 percentage points) higher proportion of controls than experimentals were working full-time at the end of the first follow-up period, and a somewhat higher (8 percentage points) proportion of contract experimentals held such positions, as compared to experimentals without contract. Experimentals removed non-voluntarily from contrat were substantially (26 percentage points) more likely that those who had dropped voluntarily to be working full-time at the end of the first follow-up period, but the difference reaches only trend significance ($X^2 = 2.80$; $p<.10$), owing to the small sample size.

Among cases who had held any type of job full-time or part-time for any part of their first month, even if the job were then lost, a majority (57%) had secured the first job held either prior to or within one week subsequent to their release, and controls who found work were somewhat (12 percentage points) more fortunate than experimentals who located jobs in acquiring them swiftly.

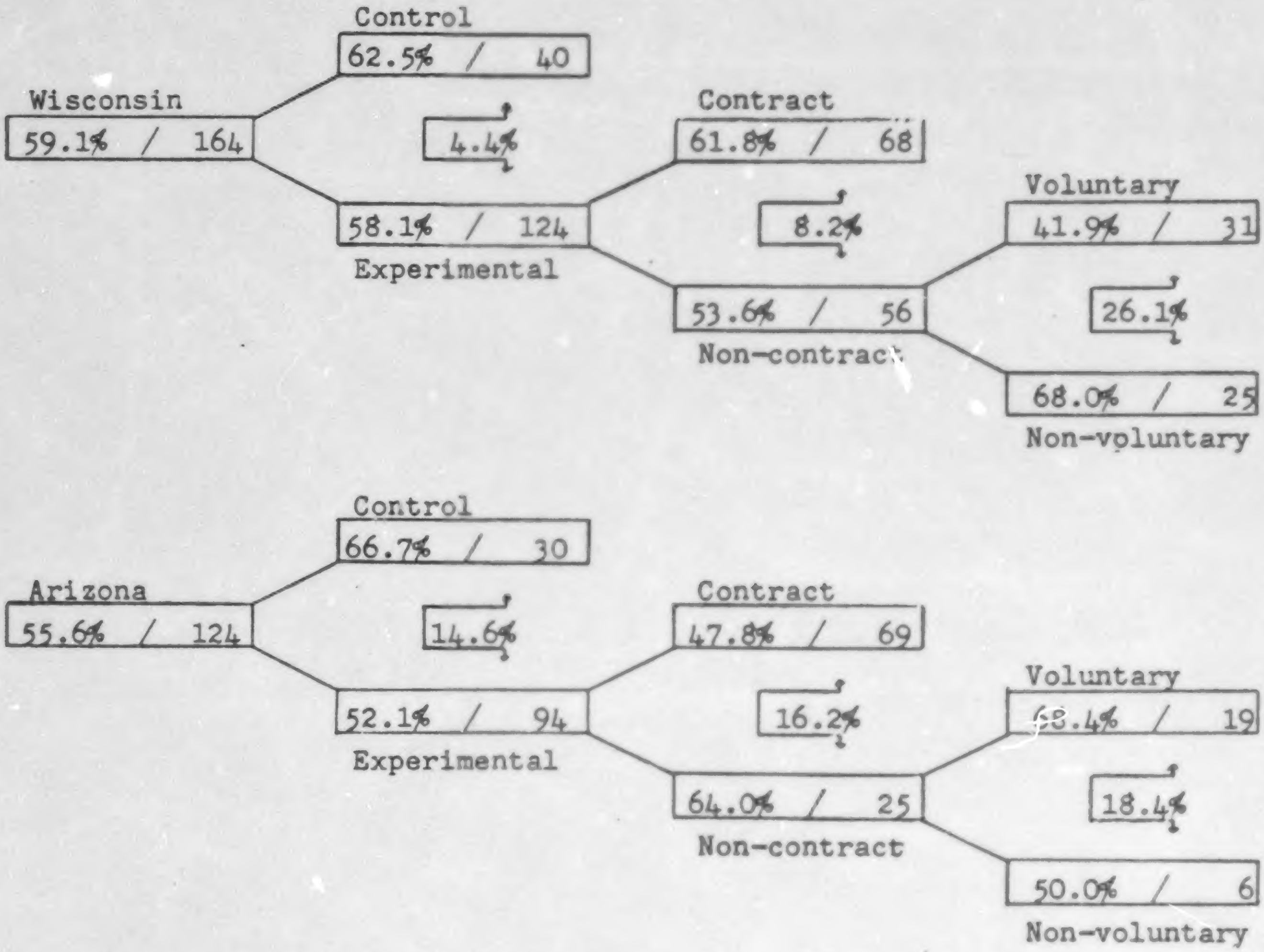
ARIZONA

Fifty-six percent of the available members of the Arizona study sample were employed full-time at the end of their first month subsequent to release. Controls were 15 percentage points more likely than experimentals to be employed full-time -- a difference which fails, however, to reach statistical significance. Within the experimental sample, a substantially higher proportion of cases who had not held MAP agreements were working full-time than were experimentals formerly under contract. Within the non-contract experimental sample, a considerably higher proportion of those who had voluntarily dropped out were in full-time positions, but the available non-voluntary subsample is extremely small.

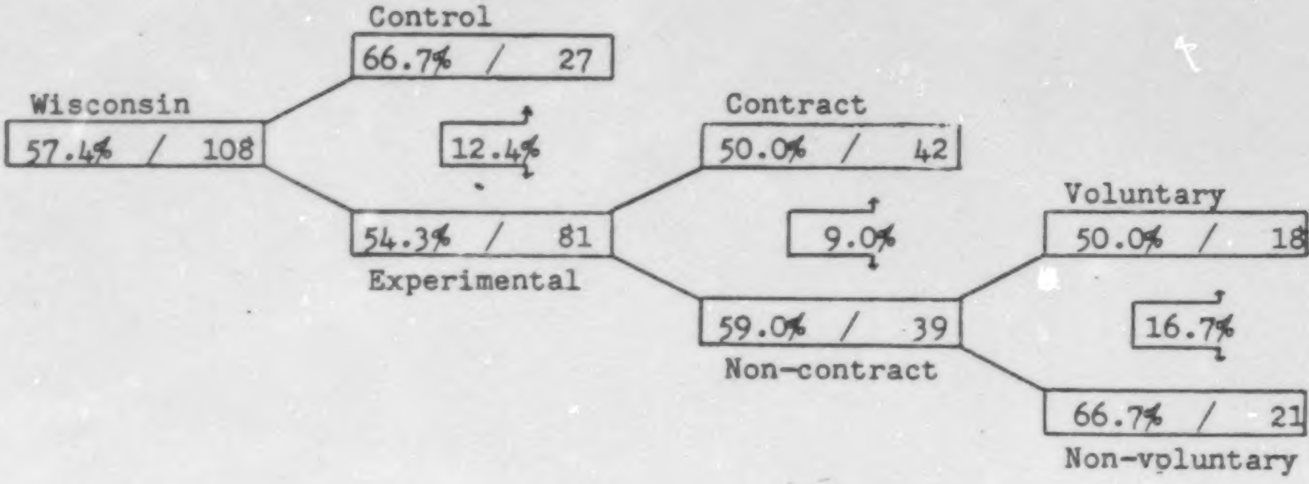
Of those who had secured any form of paid employment for any part of their first month in the community, experimentals who located work were more likely than controls to find it quickly, and there was no difference between contract and non-contract experimentals.

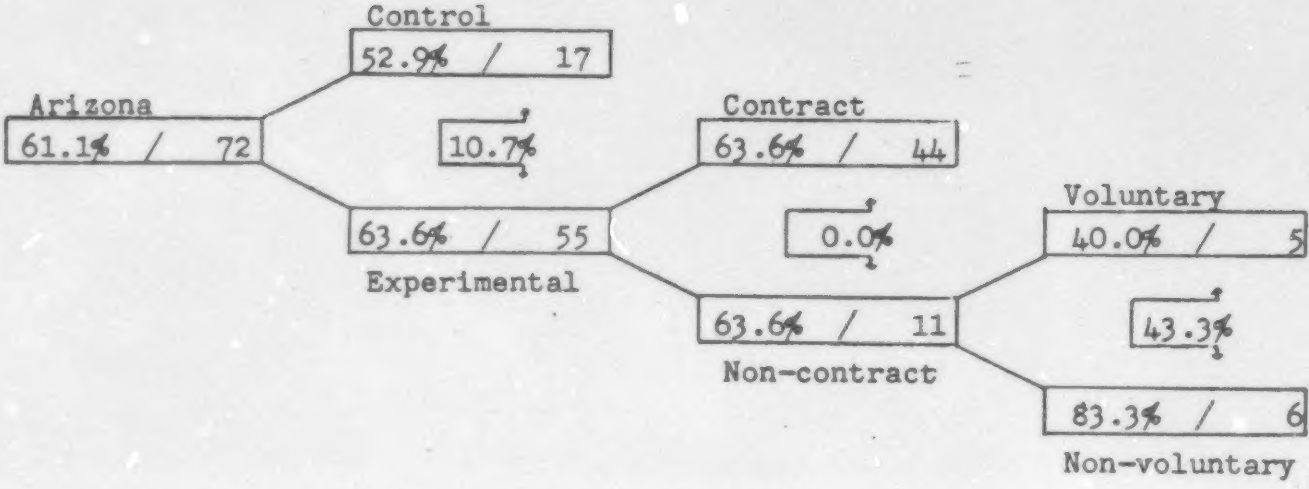
Not all subjects in either Arizona or Wisconsin were, of course, seeking full-time employment. The advantage enjoyed by controls over experimentals in both states does not, however, seem attributable to greater involvement on the part of experimentals in education or training programs; rather, the difference, as will be shown on later comparison, is largely accounted for by simple unemployment. As with recidivism data, findings for Wisconsin regarding employment and earnings are based on a more thorough data supply for released cases than findings for Arizona.

44 POST-RELEASE
EMPLOYED FULL-TIME AT ONE MONTH SUBSEQUENT TO RELEASE


Control
62.5% / 40
Wisconsin
59.1% / 164
4.4%
Contract
61.8% / 68
58.1% / 124
Experimental
8.2%
Voluntary
41.9% / 31
53.6% / 56
Non-contract
26.1%
68.0% / 25
Non-voluntary
Control
66.7% / 30
Arizona
55.6% / 124
14.6%
Contract
47.8% / 69
52.1% / 94
Experimental
16.2%
Voluntary
68.4% / 19
64.0% / 25
Non-contract
18.4%
50.0% / 6
Non-voluntary

45 POST-RELEASE
JOB OBTAINED IN LESS THAN ONE WEEK AFTER RELEASE


Wisconsin
57.4% / 108
Control
66.7% / 27
12.4%
54.3% / 81
Experimental
Contract
50.0% / 42
9.0%
59.0% / 39
Non-contract
Voluntary
50.0% / 18
16.7%
66.7% / 21
Non-voluntary


Arizona
61.1% / 72
Control
52.9% / 17
10.7%
63.6% / 55
Experimental
Contract
63.6% / 44
0.0%
63.6% / 11
Non-contract
Voluntary
40.0% / 5
43.3%
83.3% / 6
Non-voluntary

ACCESS TO EMPLOYMENT AND STABILITY OF EMPLOYMENT LEVEL OVER THREE AND SIX MONTH EXPOSURE PERIODS

WISCONSIN

The proportion of subjects employed full-time fell by one percentage point between the end of the first month and the end of the three month follow-up period, and by another six month follow-up. These are based on aggregate comparisons which do not include subjects in confinement or in absconder status in the base figure for a given period. Controls increased their advantage over experimentals in the proportion employed full-time to 9 percentage points at the end of three months following release, and the difference remained 9 percentage points in their favor at the end of the sixth month. Experimentals without contract held a 7 percentage point edge at the three month check over those who had completed contracts, but this difference had vanished at the end of six months. Nonvoluntary drops from contract consistently outperformed voluntary drops on this measure, and held a significant lead ($X^2 = 4.98$; $p<.05$) at the close of the six month period.

ARIZONA

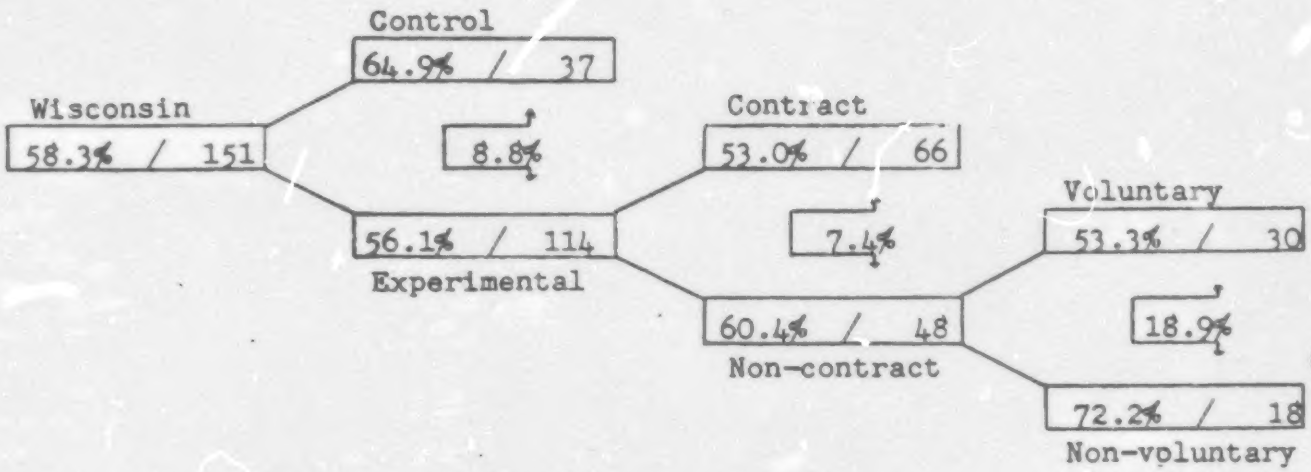
The proportion employed full-time in Arizona rose by 10 percentage points between the one month and the three month follow-up periods, and by another 4 percentage points at the end of the six month period. This improvement occurred almost totally within the experimental group, which increased its proportion employed full-time by 12 percentage points between one and three months, and by another 8 percentage points at six months; controls showed a substantial drop (12 percentage points) in full-time employment between three and six month exposure periods. The final difference of 17 percentage points between experimentals and controls after six months does not reach the threshold for statistical significance, and is based upon a small number of control cases with sufficient exposure period.

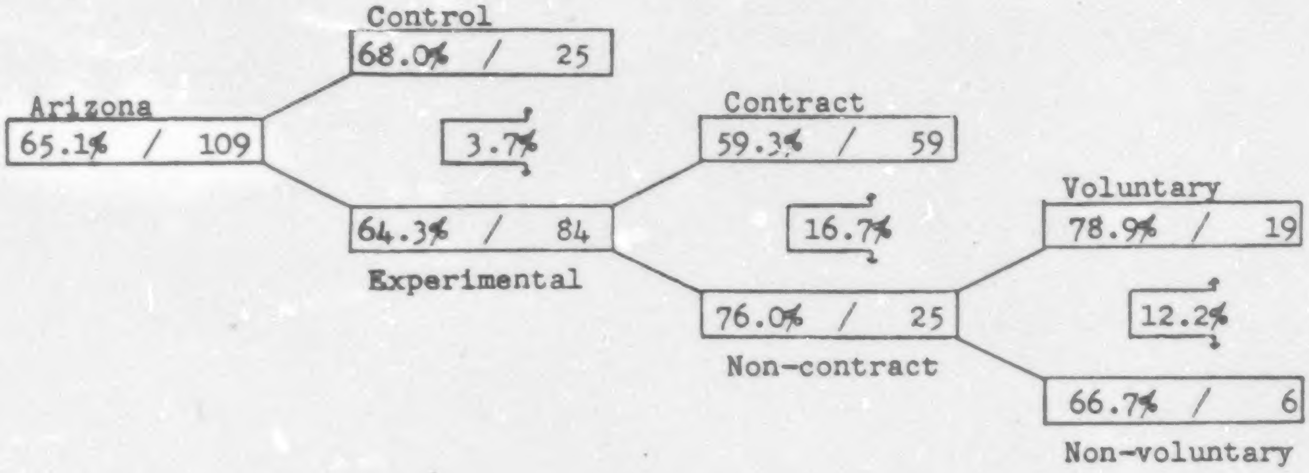
Experimentals without contract held their initial advantage over those with contract at the three month check, but the situation was reversed by the end of six months, with those formerly under contract holding a 12 percentage point lead in percent employed full time.

Comparisons made for the longer exposure periods must be considered less reliable in terms of differences found not only because of the greater loss of data which were due, but also because the number of cases upon which data were due, and their likelihood of being representative of the full comparison group involved, both become less for the longer exposure periods, since comparisons could only be made for the earliest releases. For these reasons, discussion couched in terms of change over time, but based on aggregate comparisons, tend to be misleading. Some, but not all these difficulties can be avoided by use of individual change matrices, which follow.

46 POST-RELEASE

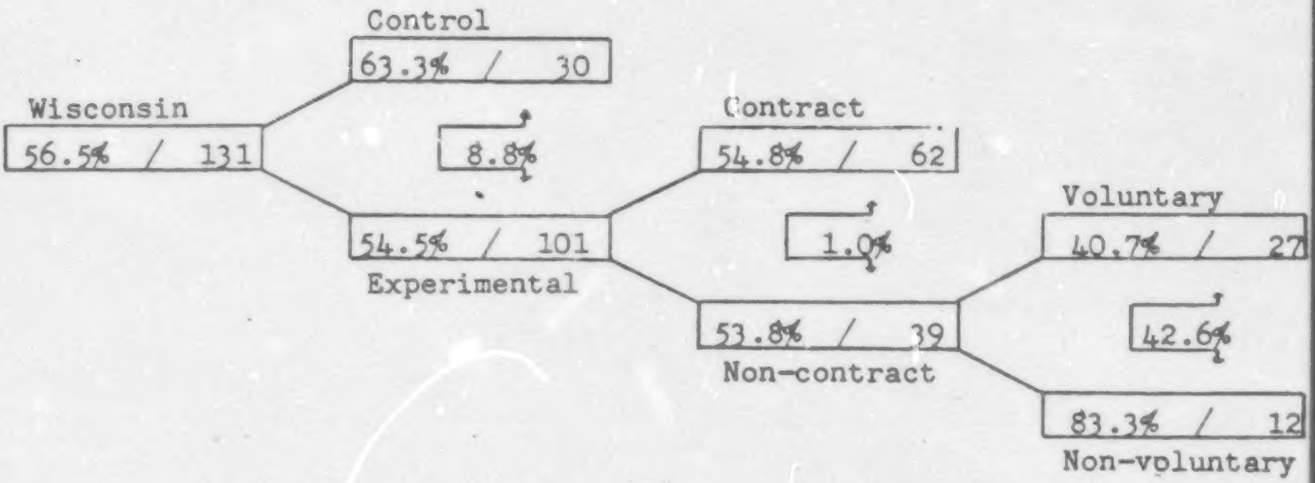
EMPLOYED FULL-TIME AT END OF THIRD MONTH FOLLOWING RELEASE

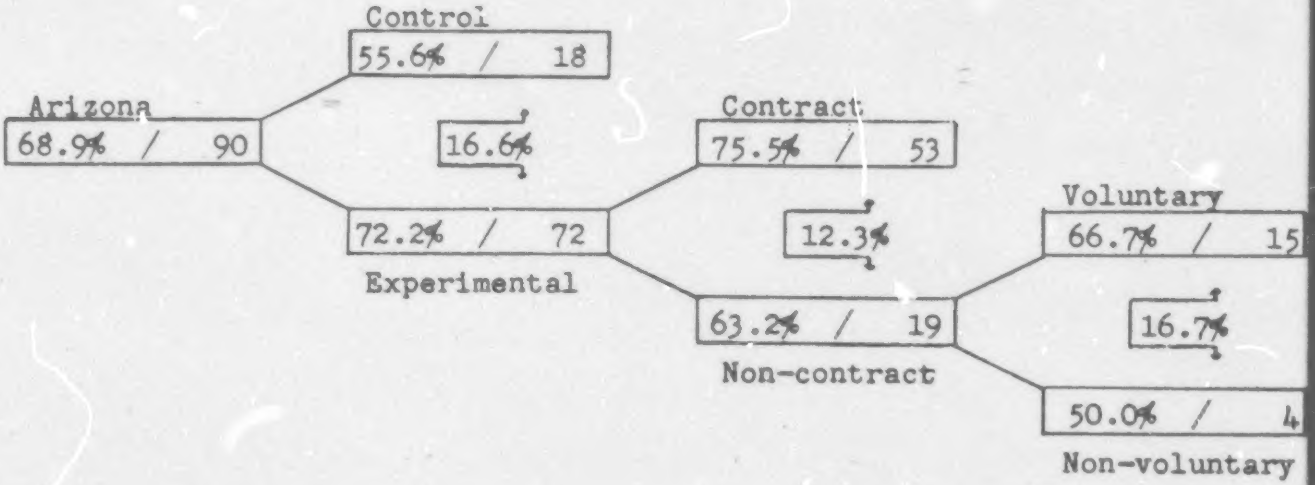

Wisconsin
58.3% / 151
Control
64.9% / 37
8.8%
Experimental
56.1% / 114
Contract
53.0% / 66
7.4%
Non-contract
60.4% / 48
Voluntary
53.3% / 30
18.9%
Non-voluntary
72.2% / 18


Arizona
65.1% / 109
Control
68.0% / 25
3.7%
Experimental
64.3% / 84
Contract
59.3% / 59
16.7%
Non-contract
76.0% / 25
Voluntary
78.9% / 19
12.2%
Non-voluntary
66.7% / 6

47 POST-RELEASE

EMPLOYED FULL-TIME AT END OF SIXTH MONTH FOLLOWING RELEASE


Wisconsin
56.5% / 131
Control
63.3% / 30
8.8%
54.5% / 101
Experimental
Contract
54.8% / 62
1.0%
53.8% / 39
Non-contract
Voluntary
40.7% / 27
42.6%
83.3% / 12
Non-voluntary


Arizona
68.9% / 90
Control
55.6% / 18
16.6%
72.2% / 72
Experimental
Contract
75.5% / 53
12.3%
63.2% / 19
Non-contract
Voluntary
66.7% / 15
16.7%
50.0% / 4
Non-voluntary

EMPLOYMENT STATUS AND PATTERNS OF MOVEMENT AMONG SURVIVORS

WISCONSIN AND ARIZONA

The following matrices are based on measures for the same individuals taken at two points in time, with the vertical axis representing employment status at the end of one month, and the horizontal at the end of three months subsequent to release. Persons in the same status at both check points fall in the cells along the table diagonal that are underlined. The tables combine entries for the Wisconsin and Arizona study samples, and represent 247 subjects who were neither confined nor at large at either the one month or three month check. Cell entries are corner-percentaged, summing to 100% for the entire table.

Persons employed full-time at both checks account for 45.7% of all experimentals. The next highest cell entry in the experimental table, accounting for 12.2% of the subjects, represents those who were unemployed at both points in time. Experimental subjects who shift from unemployed to fully employed status -- 9.1% -- have their contribution to the experimental employment level nearly cancelled by those 8.6% who shift from fully employed to unemployed. No other specific type of shift affects as much as 4% of the experimentals, and 67% of all experimentals is a 2.1 percentage point increase in the number of persons employed full-time, a 3.5 percentage point reduction in number unemployed, and a 1.5 percentage point increase in the number of students.

The pattern for controls is fairly similar to that for experimentals. Sixty-five percent show no change in status over the time period, and persons employed full-time on both occasions represent 51.7% of all controls. Movement between unemployed and fully employed status would yield a net increase of 5 percentage points in full-time employment, but this is offset by a higher rate of movement from full-time to part-time than from part-time to full-time. The number employed full-time remains constant, and the major net effect is a 6.6 percentage point increase in the number employed part-time, and a 6.7 percentage point reduction in the number of unemployed.

Comparing experimentals with controls, differences between the two samples in each status are altered very slightly between the first and third month, as can be seen in the "Experimental - Control Differences" following the next

TABLE 13

EMPLOYMENT STATUS AT ONE AND THREE MONTHS SUBSEQUENT TO RELEASE
ARIZONA AND WISCONSIN COMBINED

EXPERIMENTAL AT THREE MONTHS

		FULL	PART	UNEMP	STUDT	DISAB	WELFR	1MO	3MO	DIFF
X AT ONE MONTH	FULL	45.7%	1.5%	8.6%	1.5%	.5%		57.8%	59.9%	+2.1%
	PART	3.6	3.0		.5			7.1	7.0	-0.1
	UNEMP	9.1	2.0	12.2	2.0			25.3	21.8	-3.5
	STUDT	1.5	.5	1.0	3.6			6.6	8.1	+1.5
	DISAB					2.0		2.0	2.5	+0.5
	WELFR				.5		.5	1.0	.5	-0.5
								100.0%	100.0%	

CONTROL AT THREE MONTHS

		FULL	PART	UNEMP	STUDT	DISAB	WELFR	1MO	3MO	DIFF
C AT ONE MONTH	FULL	51.7%	8.3%	5.0%	3.3%			68.3%	68.4%	+0.1%
	PART	1.7	3.3					5.0	11.6	+6.6
	UNEMP	10.0		5.0	1.7			16.7	10.0	-6.7
	STUDT	3.3			5.0			8.3	10.0	+1.7
	DISAB	1.7				___		1.7		-1.7
	WELFR						___			
								100.0%	100.0%	

pair of matrices. These matrices compare status constancy and shift for individuals between one and six month follow-up periods, and are based on a total of 170 subjects from the combined Wisconsin and Arizona study samples.

Fifty-seven percent of experimentals and sixty-two percent of controls show no shift between the two o-casions. Among experimentals, 42.4% are fully employed both times, and the same is true for 47.9% of controls. About 10% of each comparison group is unemployed on both occasions, and movement back and forth between full-time employment and unemployed are the major categories of shift in both comparison groups, with movement from full-time employment to student status in the control sample being the only other shift involving as many as 5% of either comparison group.

In terms of net effects within the comparison groups and changes in the pattern of differences between them, experimentals show a net gain of 4.8 percentage points in full-time employment, a net reduction of 5.8 percentage points in unemployment, and minimal change in the other status categories. The major net changes for controls are an 8.3 percentage point gain in the number of students. Between the first and sixth months, experimentals wiped out the early employment gap between themselves and controls and ended with an advantage of 1.7 percentage points in full-time employed and 4.3 percentage points in part-time employed; during that same period, however, the initial 1.7 percentage point disadvantage in number of students grew to one of 7.2 percentage points as compared to controls.

TABLE 14

EMPLOYMENT STATUS AT ONE AND SIX MONTHS SUBSEQUENT TO RELEASE
ARIZONA AND WISCONSIN COMBINED

EXPERIMENTAL AT SIX MONTHS

		FULL	PART	UNEMP	STUDT	DISAB	WELFR	1MO	6MO	DIFF
C AT ONE MONTH	FULL	42.4%	2.3%	9.3%	1.7%	1.7%		57.4%	62.2%	+4.8%
	PART	2.9	1.7	1.2	.6	.6		7.0	6.4	-0.6
	UNEMP	12.8	1.2	8.7	1.7	.6		25.0	19.2	-5.8
	STUDT	3.5	1.2		2.9			7.6	7.5	-0.1
	DISAB	.6				1.7		2.3	4.6	+2.3
	WELFR				.6		—	.6	—	- .6
								100.0%	100.0%	

CONTROL AT SIX MONTHS

		FULL	PART	UNEMP	STUDT	DISAB	WELFR	1MO	6MO	DIFF
C AT ONE MONTH	FULL	47.9%	2.1%	10.4%	6.3%	2.1%		68.8%	60.5%	-8.3%
	PART	2.1	—		2.1			4.2	2.1	-2.1
	UNEMP	6.3		10.4	2.1			18.8	20.8	+2.0
	STUDT	2.1			4.2			6.3	14.7	+8.4
	DISAB	2.1				—		2.1	2.1	+4.2
	WELFR						—			
								100.0%	100.0%	

EXPERIMENTAL-CONTROL DIFFERENCES

	FULL	PART	UNEMP	STUDT	DISAB	WELFR
AT ONE MONTH	-10.5	- 2.1	+ 8.6	- 1.7	+ 0.3	+ 1.0
AT THREE MONTHS	- 8.5	- 4.6	+11.8	- 1.9	+ 2.5	+ 0.5
AT SIX MONTHS	+ 1.7	+ 4.3	- 1.6	- 7.2	+ 2.5	+ 0.6

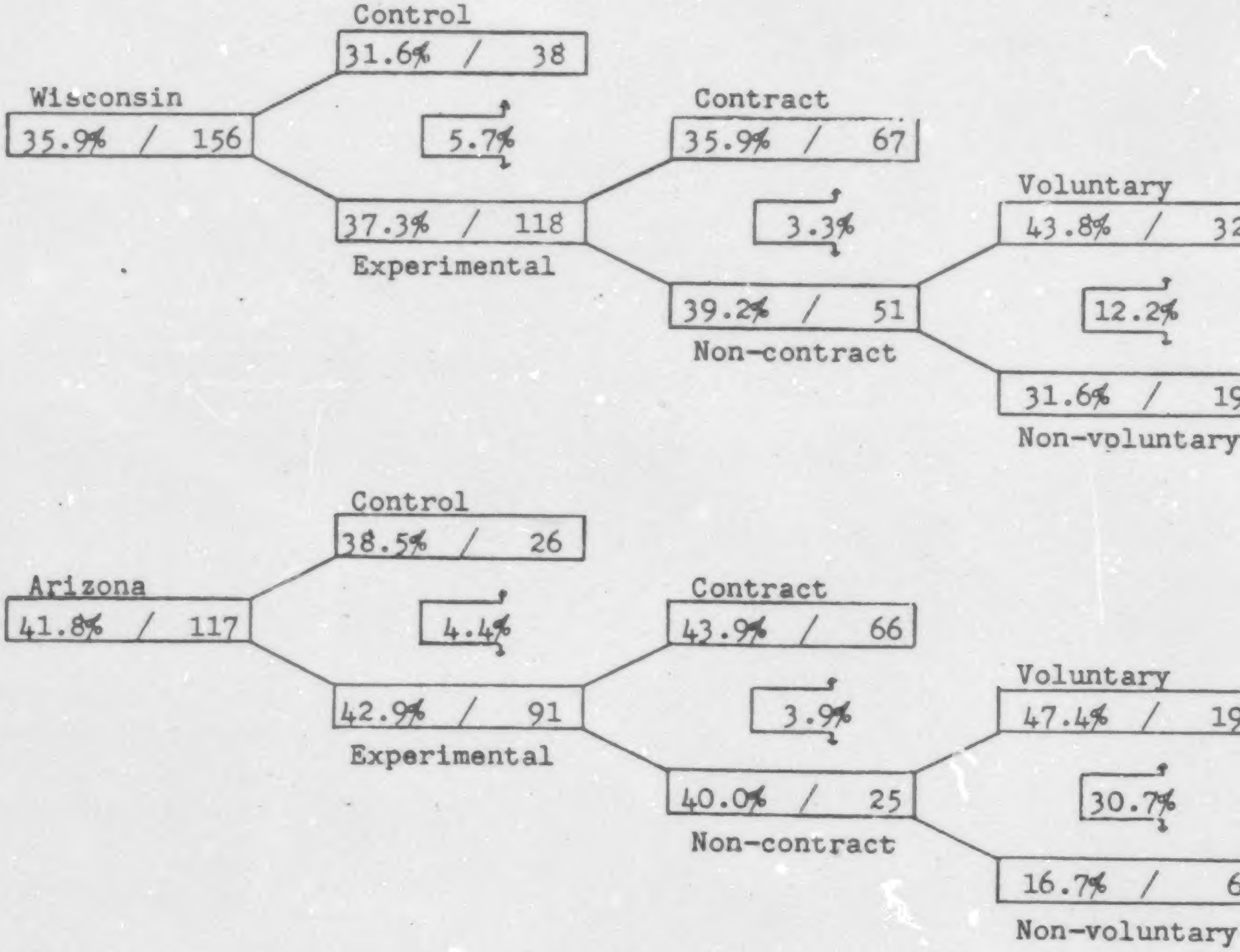
CHANGES IN JOB POSITION DURING FOLLOW-UP PERIOD

WISCONSIN AND ARIZONA

In the preceding sections, findings indicated relatively low net changes between follow-up periods in the proportion of subjects free in the community who were employed, a sizable core of cases who were stable in maintaining full-time employment, and most of the changes in status which occurred consisting of transfers back and forth between the categories of full-time employment and unemployment. These findings, however, give no direct indication of the extent of movement between jobs; individuals, for example, could have been employed full-time at the end of comparison periods, but unemployed for some part of the interim, or vice versa, and individuals could have continuously held full-time positions, but changed from one job to another.

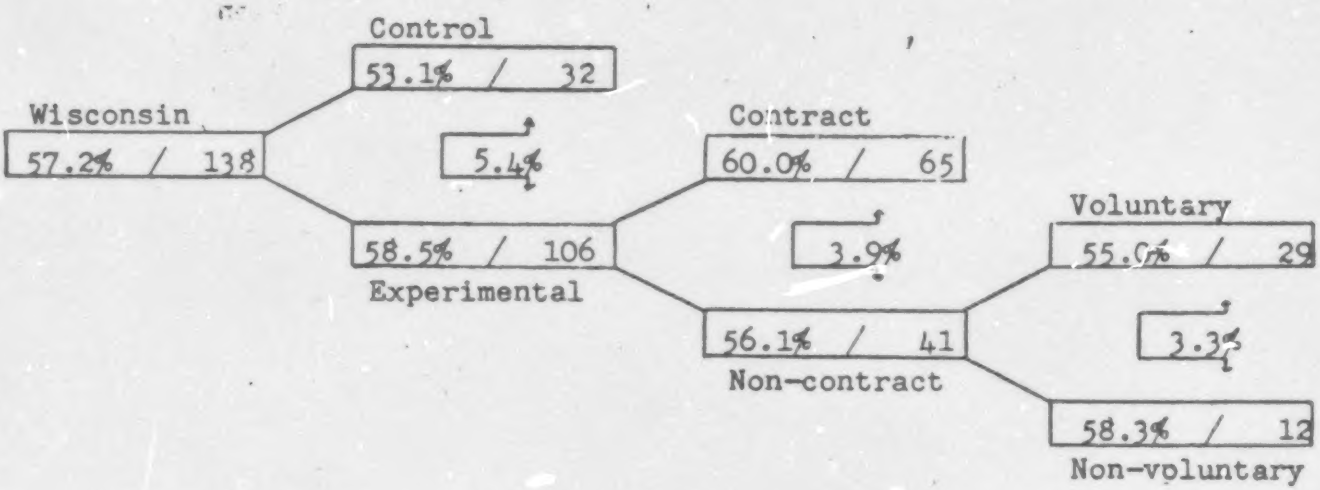
Subjects were asked at one month, three months, and six months following release how many jobs they had held since release, regardless of whether they were currently employed. By the end of three months 36% of respondents in Wisconsin and 42% in Arizona claimed they had had two or more different jobs since their release. By the end of six months, 57% in each state claimed two or more different jobs following release. Differences between experimentals and controls in reply to this question were quite slight in both states for both follow-up periods. Differences were were similarly slight between contract and non-contract experimentals with the exception that, in Arizona, six month comparison revealed substantially (though not significantly) greater movement among those who had completed contracts.

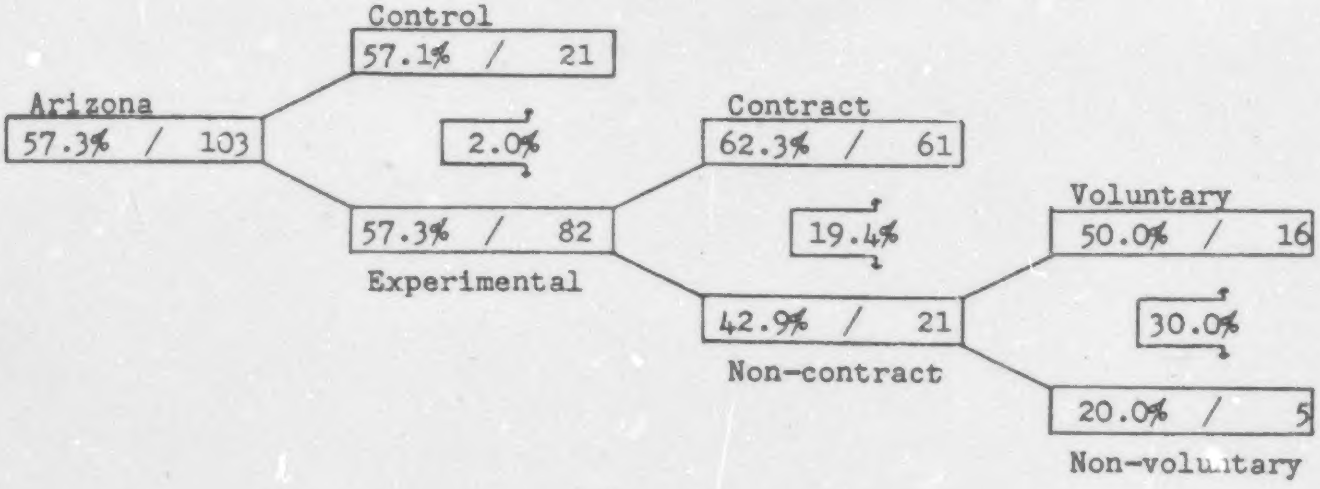
48 POST-RELEASE
RELEASEE HELD TWO OR MORE DIFFERENT JOBS WITHIN THREE MONTHS SUBSEQUENT TO RELEASE


Wisconsin
35.9% / 156
Control
31.6% / 38
5.7%
37.3% / 118
Experimental
Contract
35.9% / 67
3.3%
39.2% / 51
Non-contract
Voluntary
43.8% / 3
12.2%
31.6% / 1
Non-voluntary
Arizona
41.8% / 117
Control
38.5% / 26
4.4%
42.9% / 91
Experimental
Contract
43.9% / 66
3.9%
40.0% / 25
Non-contract
Voluntary
47.4% / 1
30.7%
16.7% / 6
Non-voluntary

49 POST-RELEASE

RELEASEE HELD TWO OR MORE DIFFERENT JOBS WITHIN SIX MONTHS SUBSEQUENT TO RELEASE


Wisconsin
57.2% / 138
Control
53.1% / 32
5.4%
58.5% / 106
Experimental
Contract
60.0% / 65
3.9%
56.1% / 41
Non-contract
Voluntary
55.0% / 29
3.3%
58.3% / 12
Non-voluntary


Arizona
57.3% / 103
Control
57.1% / 21
2.0%
57.3% / 82
Experimental
Contract
62.3% / 61
19.4%
42.9% / 21
Non-contract
Voluntary
50.0% / 16
30.0%
20.0% / 5
Non-voluntary

RELEVANCE OF PRISON TRAINING TO JOB OBTAINED AFTER RELEASE

WISCONSIN

For each follow-up period subjects were asked, concerning their current job if employed and their best job after release if unemployed, how much of their preparation for that kind of job had been obtained during their last prison stay.

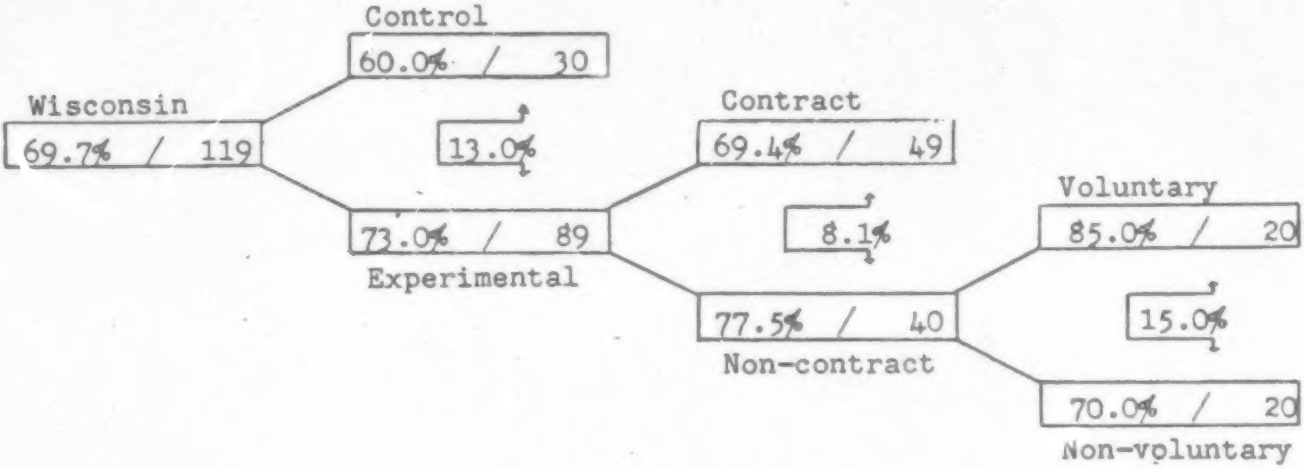
A high majority of former Wisconsin prisoners who obtained jobs -- between 70% and 73% at each exposure period -- claimed that very little or none of the preparation for their post-release job had been obtained in prison. At one month, controls were 13 percentage points more likely than experimentals to claim more than "very little" relevant prison preparation, but at three months they were 10 percentage points less likely to do so, and at six months equally likely; the differences are not significant. Contract experimentals were consistently more likely than those without contract to claim more than very little relevant preparation and, for the three-month follow-up period, this difference passed the threshold for trend significance ($X^2 = 2.89$; $p < .10$).

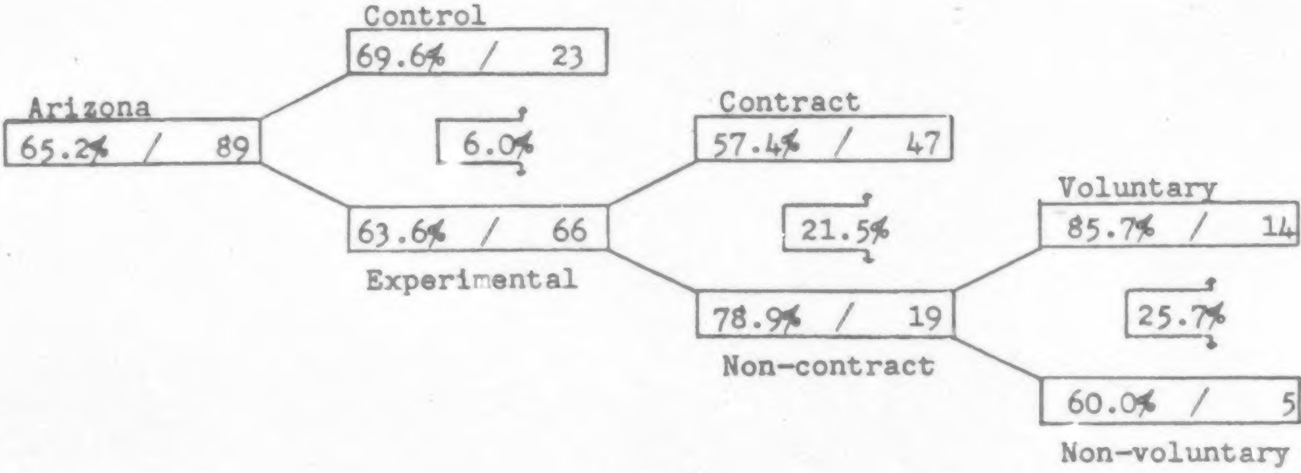
ARIZONA

At one month, 65% of Arizona releasees who reported having held a job attributed very little or no relevance to that job of preparations in prison. This opinion spread to be shared by 76% of subjects after three months in the community, and remained at 76% on six month follow-up.

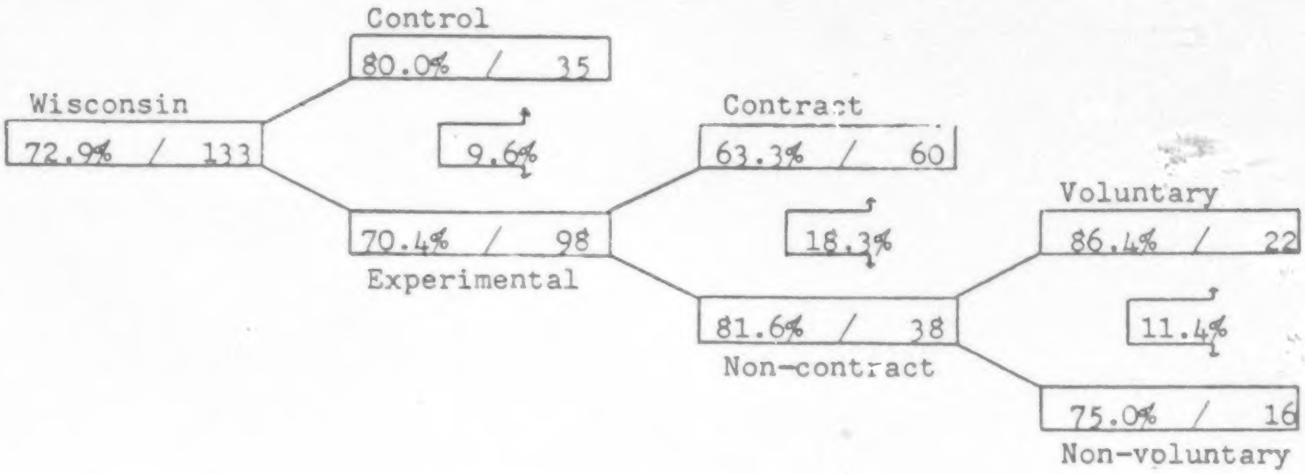
At one month, Arizona controls were 6 percentage points less likely than experimentals to view prison preparation as having more than very little relevance to their job in the community, but at three months this difference evaporated, and by six months, they were 12 percentage points more likely than experimentals to claim relevant preparation; these differences are not likely than those without contract to claim relevant prison preparation, but the difference was not established at a statistically significant level for any comparison period. Contract experimentals were hardly more likely than controls to claim job-relevant prison preparation three months subsequent to release, and were slightly less likely than controls to do so at six months.

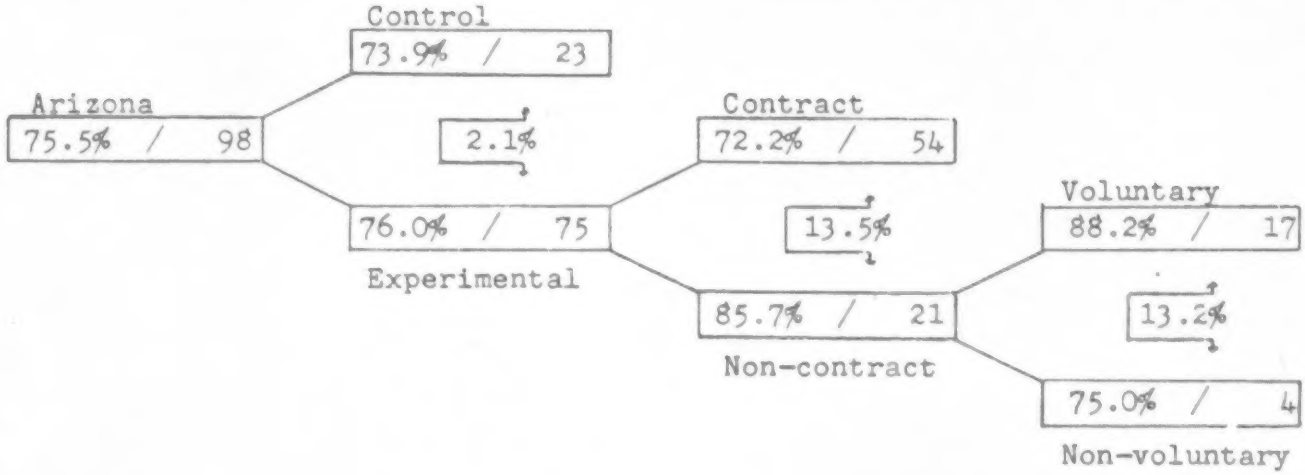
50 POST-RELEASE ONE MONTH
RELEASEE CLAIMED VERY LITTLE OR NONE OF TRAINING FOR CURRENT OR BEST JOB WAS OBTAINED DURING LAST PRISON STAY


Wisconsin
69.7% / 119
Control
60.0% / 30
13.0%
73.0% / 89
Experimental
Contract
69.4% / 49
8.1%
77.5% / 40
Non-contract
Voluntary
85.0% / 20
15.0%
70.0% / 20
Non-voluntary


Arizona
65.2% / 89
Control
69.6% / 23
6.0%
63.6% / 66
Experimental
Contract
57.4% / 47
21.5%
78.9% / 19
Non-contract
Voluntary
85.7% / 14
25.7%
60.0% / 5
Non-voluntary

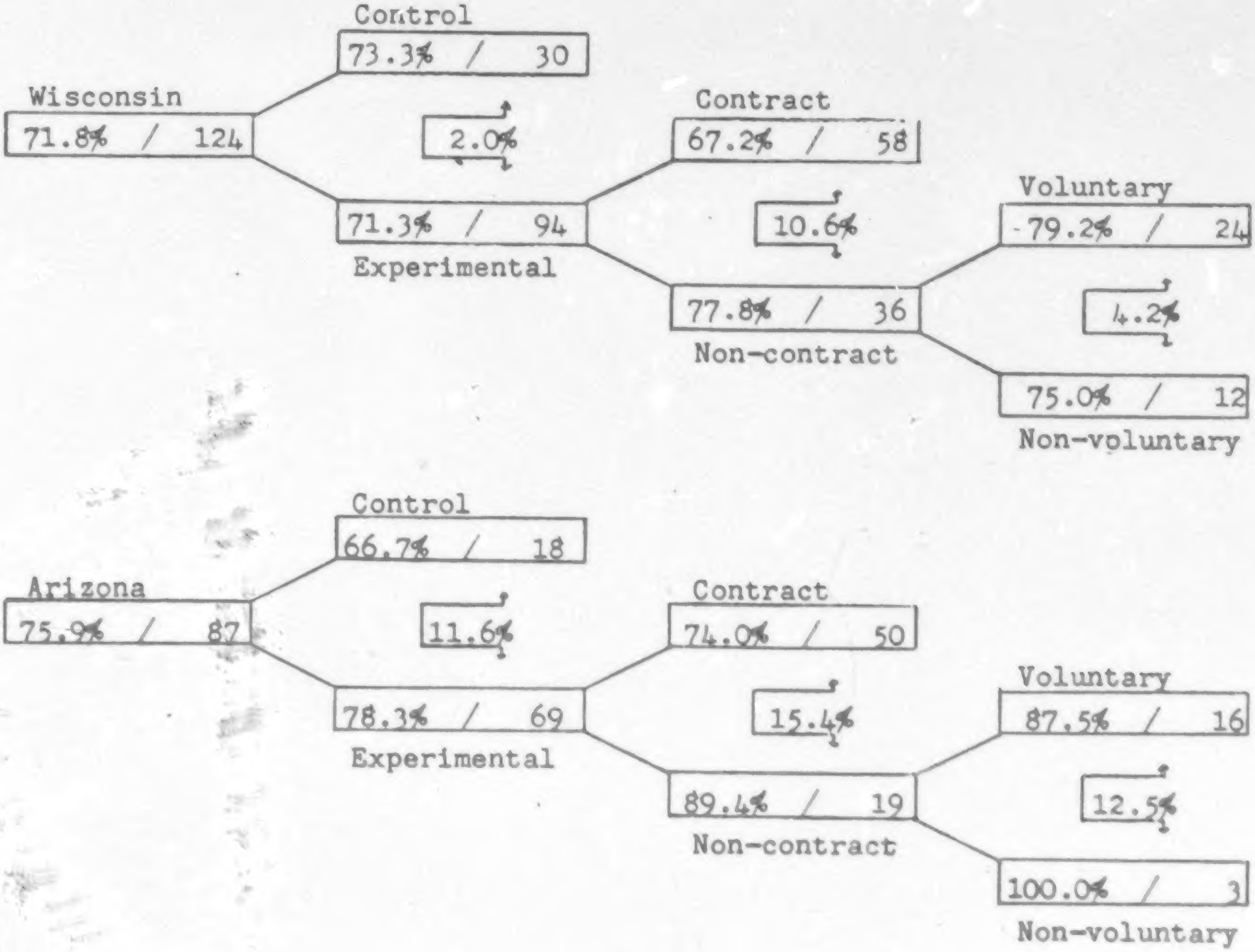
51 POST-RELEASE THREE MONTHS
RELEASEE CLAIMED VERY LITTLE OR NONE OF TRAINING FOR CURRENT OR BEST JOB WAS OBTAINED DURING LAST PRISON STAY


Wisconsin
72.9% / 133
Control
80.0% / 35
9.6%
70.4% / 98
Experimental
Contract
63.3% / 60
18.3%
81.6% / 38
Non-contract
Voluntary
86.4% / 22
11.4%
75.0% / 16
Non-voluntary


Arizona
75.5% / 98
Control
73.9% / 23
2.1%
76.0% / 75
Experimental
Contract
72.2% / 54
13.5%
85.7% / 21
Non-contract
Voluntary
88.2% / 17
13.2%
75.0% / 4
Non-voluntary

52 POST-RELEASE SIX MONTHS

RELEASEE CLAIMED VERY LITTLE OR NONE OF TRAINING FOR CURRENT OR BEST JOB WAS OBTAINED DURING LAST PRISON STAY



Wisconsin
71.8% / 124
Control
73.3% / 30
2.0%
71.3% / 94
Experimental
Contract
67.2% / 58
10.6%
77.8% / 36
Non-contract
Voluntary
79.2% / 24
4.2%
75.0% / 12
Non-voluntary
Arizona
75.9% / 87
Control
66.7% / 18
11.6%
78.3% / 69
Experimental
Contract
74.0% / 50
15.4%
89.4% / 19
Non-contract
Voluntary
87.5% / 16
12.5%
100.0% / 3
Non-voluntary

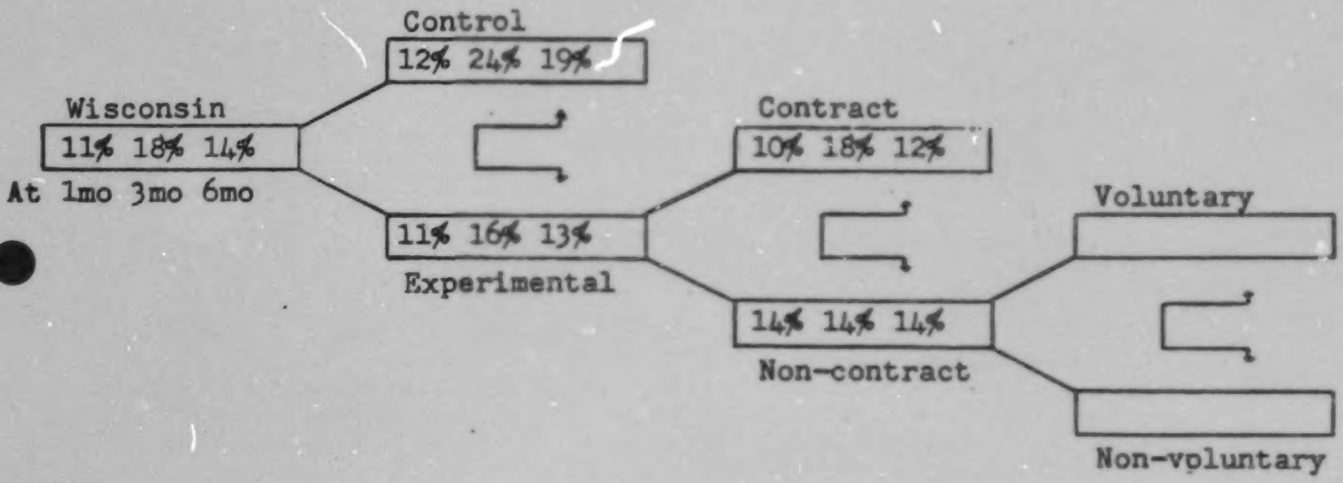
TRAINING OBTAINED SUBSEQUENT TO RELEASE

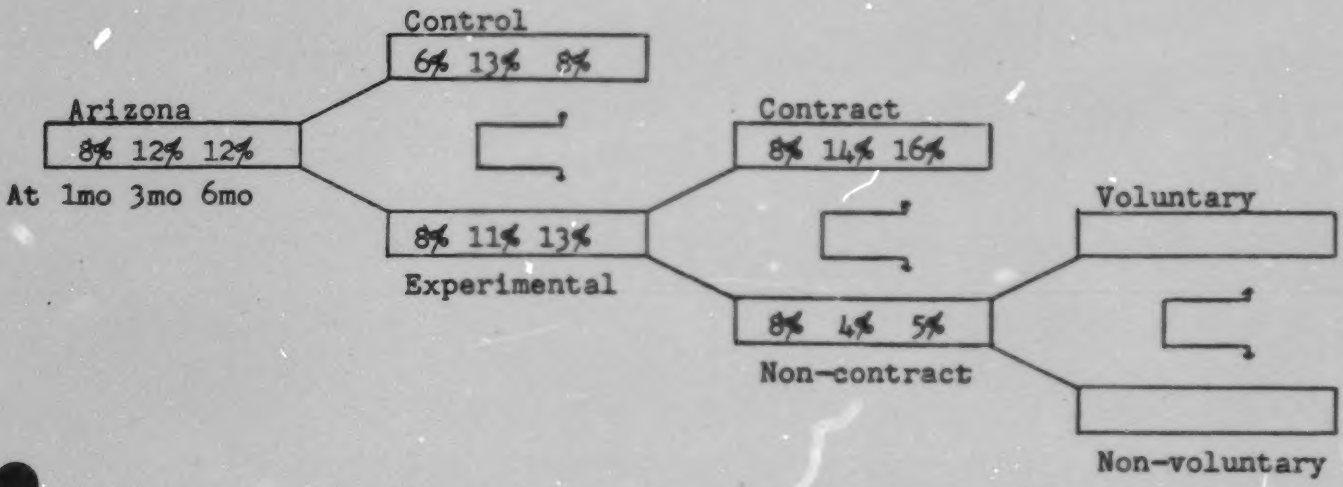
WISCONSIN AND ARIZONA

Inquiry was made of each case on whom follow-up information was obtained about whether the releasee had been enrolled at any time subsequent to release in any education or job training course or program. Positive replies were obtained for 11% of subjects in Wisconsin at the end of the one month follow-up period, for 18% at three months, and for 14% at six months. The drop in percent between three and six month follow-up periods on a type of measure from which findings should cumulate over time is attributable to differing membership in the study sample of subjects due and available for examination by the data collection cut-off date. In Arizona, subjects claiming enrollment over the one, three, and six month exposure periods amounted to 8%, 12%, and 12% of respondents.

In the comparison of experimentals and controls on exposure to post-release training or education, raw differences never exceeded 8 percentage points in either state for any follow-up period; given the low overall likelihood of involvement, slight absolute percentage differences may appear disproportionately large. For example, at three month follow-up the enrollment rate for Arizona non-contract experimentals is less than a third of that for contract experimentals, and that for Wisconsin controls one and a half times that of experimentals, but both those differences could be expected to occur at least once in four times by chance and, even if the differences were reliable, the primary practical fact remains the same -- the proportion obtaining training after release was small across all subgroups.

53 POST-RELEASE ONE, THREE, SIX MONTH
RELEASEE CLAIMED ENROLLMENT IN SOME EDUCATION OR JOB TRAINING COURSE OR PROGRAM SUBSEQUENT TO RELEASE


Control
12% 24% 19%
Wisconsin
11% 18% 14%
At 1mo 3mo 6mo
Contract
10% 18% 12%
11% 16% 13%
Experimental
Voluntary
14% 14% 14%
Non-contract
Non-voluntary


Control
6% 13% 8%
Arizona
8% 12% 12%
At 1mo 3mo 6mo
Contract
8% 14% 16%
8% 11% 13%
Experimental
Voluntary
8% 4% 5%
Non-contract
Non-voluntary

HOURLY EARNINGS SUBSEQUENT TO RELEASE

WISCONSIN

If subjects were employed at the end of a given follow-up period, information was sought about current hourly earnings, and if they were unemployed, the highest hourly earnings for any job held at any time subsequent to release were to be reported.

In Wisconsin, 50% of the study sample who reported any job by the end of the one month follow-up claimed hourly earnings of at least \$3.00. For the subjects available for three month follow-up, there was a rise to 55%, and at the end of six months, 62% of those who had held a job reported earnings of at least \$3.00 per hour.

At one month exposure, experimentals held a 14 percentage point (statistically non-significant) advantage over controls for claims of wages of \$3.00 per hour or more. This difference diminished over the longer exposure periods, falling to 10 percentage points at three months, and to 2 percentage points at six months. At all three follow-up periods, experimentals who had completed contracts enjoyed a lead of 12 -- 20 percentage points over experimentals without contracts. The largest of these differences was found at six months, and reaches trend significance ($X^2 = 2.90$; $p < .10$). Similarly, non-voluntary drops from contract held a consistent and substantial advantage over voluntary drops, though this difference was always short of statistical significance, and declined in magnitude for the lengthiest follow-up period.

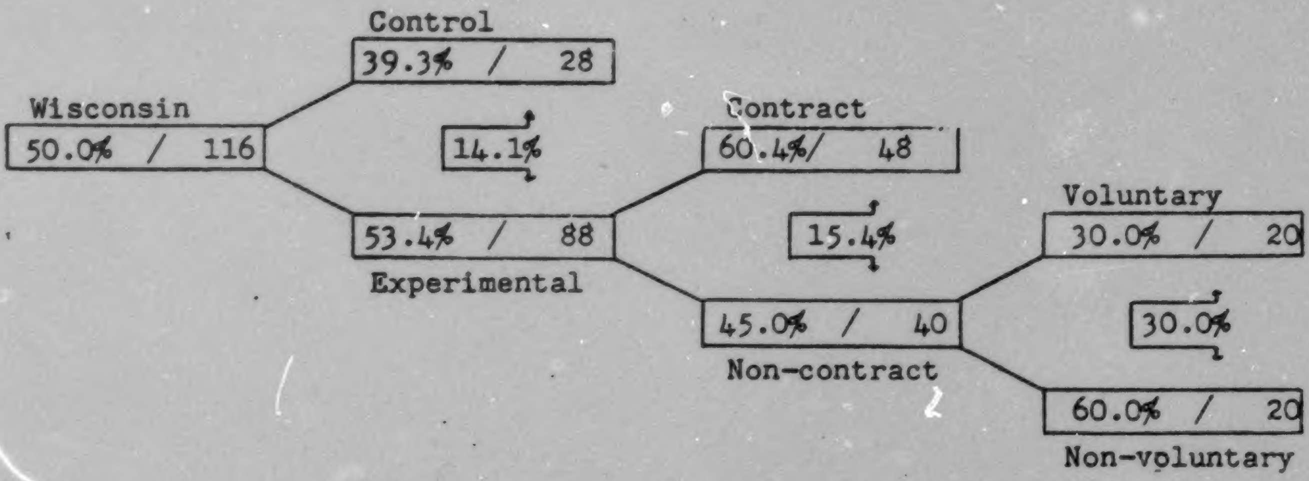
ARIZONA

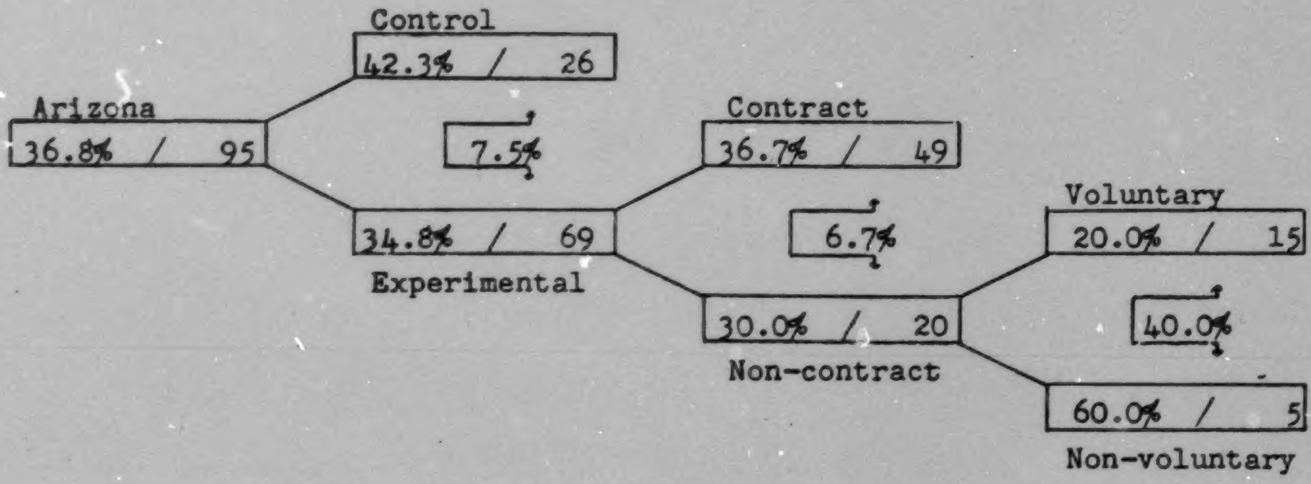
Thirty-seven percent of Arizona subjects who held a job at some time during the first month after release, and 45% who had held a job within three months after release reported an hourly wage of at least $3.00. At six month follow-up, only 41% reported having held such a job -- a finding which could result from moving to lower paying jobs and reporting current wage, but one which more probably reflects merely a difference in composition of the subset of subjects due and accessible for six month follow-up.

Controls who had held a job within a month after release were eight percent more likely than their experimental counterparts to have reached the $3.00/hour earning level, but this advantage was reversed in the subsequent at six months. Among experimentals, those who had held contracts outperformed those who had not at one month and at three months, but the direction of advantage reversed at six months exposure. None of the differences between experimentals and controls or between contract and non-contract experimentals was statistically significant.

The hourly wage findings are subject to the same types of limitation discussed in the preceding section on employment status, and an additional qualification is necessary -- percents achieving $3.00/hour or more are derived from the base figure of those who had held a job, and comparisons of percent differences fail to take into account differences in comparison group employment rates. Again, some of the ambiguities in findings can be reduced by scrutiny of change matrices.

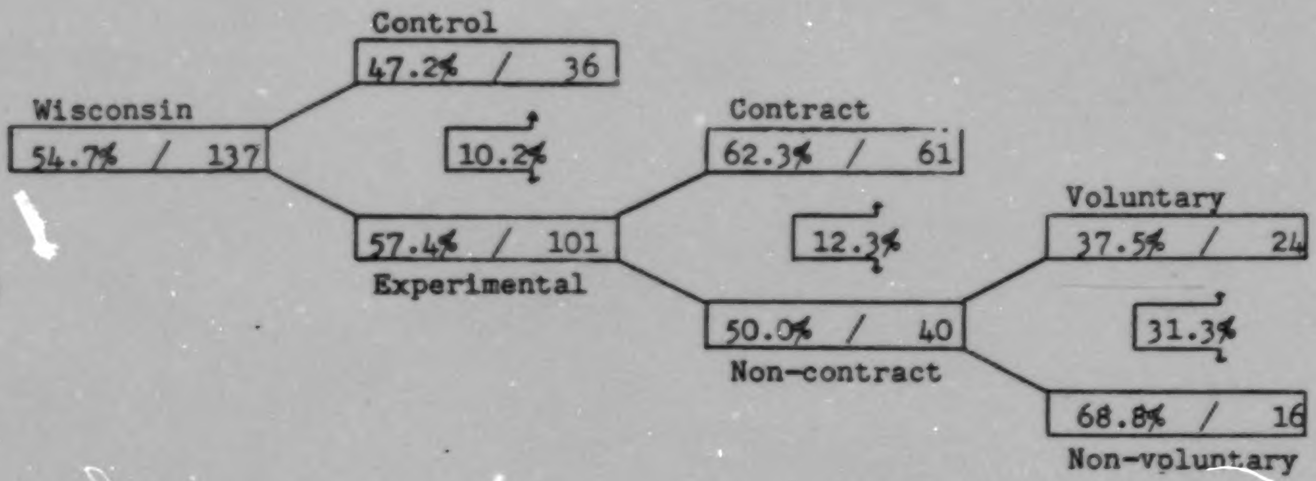
54 POST-RELEASE
RELEASEE REPORTED HOURLY WAGE REACHED AT LEAST $3.00/HR. WITHIN FIRST MONTH AFTER RELEASE

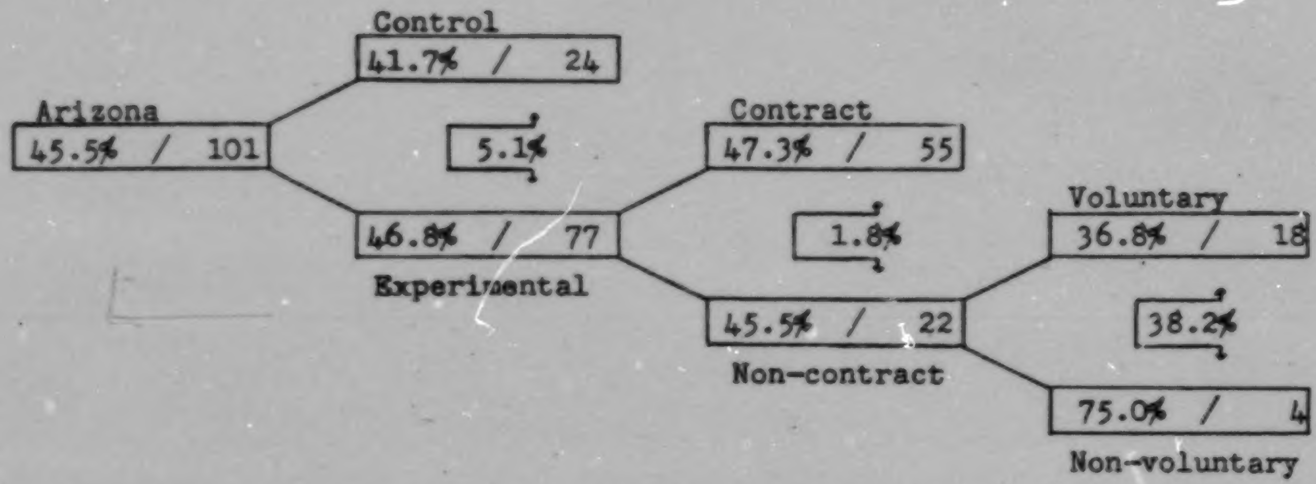

Wisconsin
50.0% / 116
Control
39.3% / 28
14.1%
53.4% / 88
Experimental
Contract
60.4%/ 48
15.4%
45.0% / 40
Non-contract
Voluntary
30.0% / 20
30.0%
60.0% / 20
Non-voluntary


Arizona
36.8% / 95
Control
42.3% / 26
7.5%
34.8% / 69
Experimental
Contract
36.7% / 49
6.7%
30.0% / 20
Non-contract
Voluntary
20.0% / 15
40.0%
60.0% / 5
Non-voluntary

55 POST-RELEASE

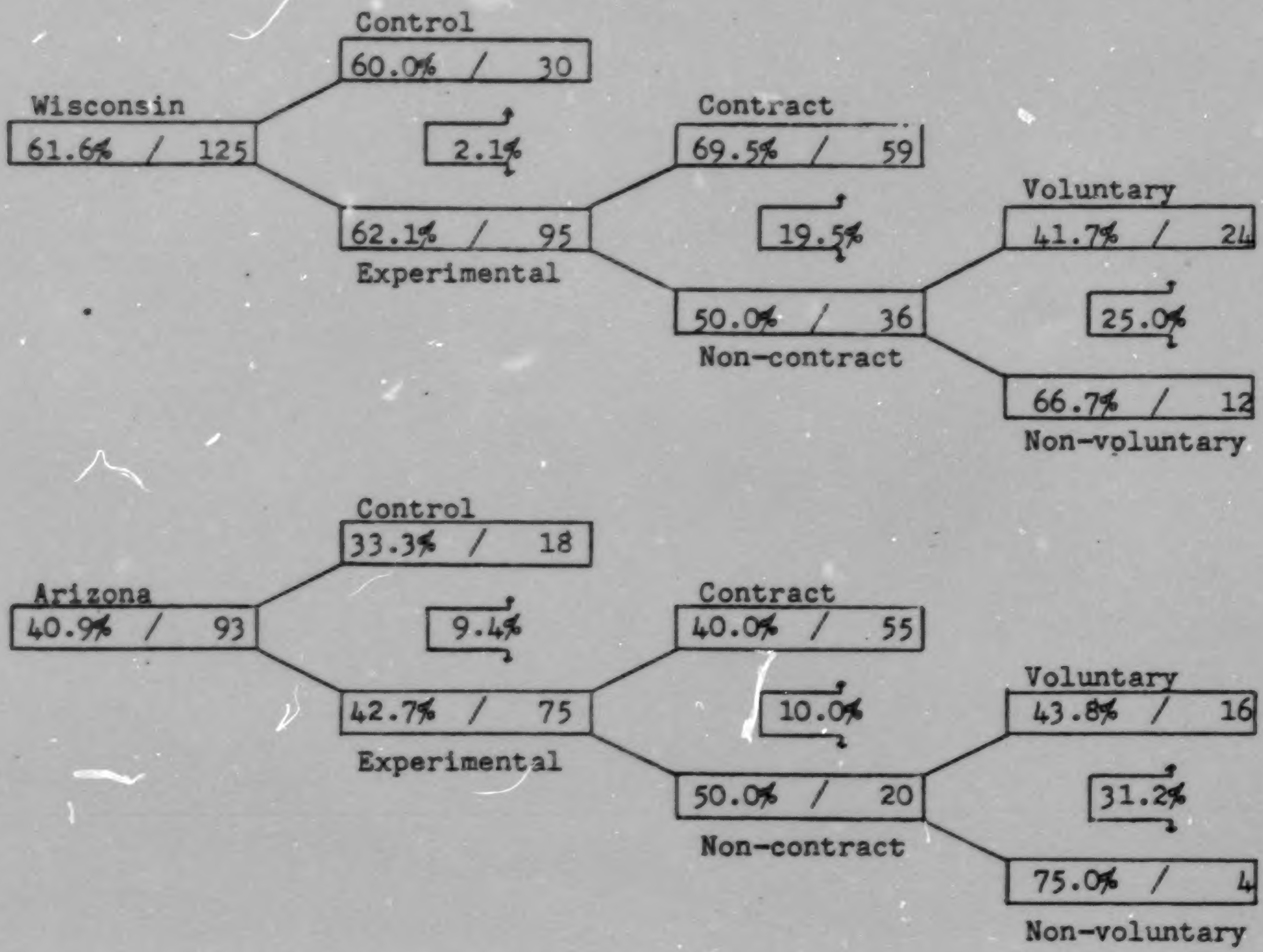
RELEASEE REPORTED HOURLY WAGE REACHED AT LEAST $3.00/HR. WITHIN THREE MONTHS AFTER RELEASE


Wisconsin
54.7% / 137
Control
47.2% / 36
10.2%
Experimental
57.4% / 101
Contract
62.3% / 61
12.3%
Non-contract
50.0% / 40
Voluntary
37.5% / 24
31.3%
Non-voluntary
68.8% / 16


Arizona
45.5% / 101
Control
41.7% / 24
5.1%
Experimental
46.8% / 77
Contract
47.3% / 55
1.8%
Non-contract
45.5% / 22
Voluntary
36.8% / 18
38.2%
Non-voluntary
75.0% / 4

56 POST-RELEASE

RELEASEE REPORTED HOURLY WAGE REACHED AT LEAST $3.00/HR. WITHIN SIX MONTHS AFTER RELEASE


Wisconsin
61.6% / 125
Control
60.0% / 30
2.1%
Experimental
62.1% / 95
Contract
69.5% / 59
19.5%
Non-contract
50.0% / 36
Voluntary
41.7% / 24
25.0%
Non-voluntary
66.7% / 12
Arizona
40.9% / 93
Control
33.3% / 18
9.4%
Experimental
42.7% / 75
Contract
40.0% / 55
10.0%
Non-contract
50.0% / 20
Voluntary
43.8% / 16
31.2%
Non-voluntary
75.0% / 4

CHANGES IN INDIVIDUAL EARNING LEVEL

WISCONSIN AND ARIZONA

The form of analysis applied in the following pages is similar to that used earlier to plot changes over time in the employment status of individuals. Again, the Wisconsin and Arizona study samples are combined, and comparison made between all experimentals and controls for whom information was available -- a technique which obscures differential contribution by the separate states to particular effects, and fails to take into account the differing ratio of experimentals to controls in the two states and its impact on findings. The technique, instead, gives equal weight to each person for whom data existed at both comparison points.

There were 150 Wisconsin experimentals and 130 Arizona experimentals in the full study sample, and 50 and 65 controls, respectively, for totals of 280 experimentals and 115 controls, yielding a 2.4:1 X to C ratio, or a study sample in which 71% of cases were experimentals. In the comparisons which follow, experimentals each time account for between 74% and 79% of cases compared, and are thus slightly over-represented. Comparisons are made, by pairs, for five different points in time:

1. Highest earning level prior to incarceration.
2. Highest earning level anticipated when released.
3. Highest or current earning level 1 month after release.
4. Highest or current earning level 3 months after release.
5. Highest or current earning level 6 months after release.

Since comparisons involving points 3, 4, or 5 require that: 1. A subject had been released sufficiently early to complete the exposure period before cessation of data collection; 2. The subject was accessible and made the necessary information available; and 3. That he had some earnings to report subsequent to release; the comparisons are available, depending upon the particular comparison, for only those 43% to 55% of the full 395 member study sample who met all conditions necessary for inclusion.

Two types of comparison were made for each person involved.

1. Did his reported hourly wage increase, decrease, or hold constant between the two comparison points?*

2. Did his reported hourly wage stay below, stay above, move from below to above, or move from above to below $3.00 per hour?

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*Constant wage does not mean constant earning power, because of inflationary effects. This is an important consideration for comparisons involving the pre-prison earning level.

EARNINGS PRIOR TO INCARCERATIONS

vs.

EARNINGS ESTIMATE MADE NEAR TIME OF RELEASE
(271 Cases)

Fifty percent of experimental cases involved in this comparison claimed they had reached or exceeded the $3.00 per hour level on some job preceding their incarceration; as release approached, 65% anticipated reaching this level during their first six months in the community -- a 15 percentage point increase. Approximately the same proportion of controls (63%) expected to attain $3.00/hour, although a lesser proportion (44%) claimed they had reached it before coming to prison; controls thus increased by 19 percentage points, comparing accomplishment to expectation, and the aspiration gap between controls and experimentals narrowed to 1 percentage point because of their greater optimism.

Hourly wages had been categorized into eight intervals, with the extreme categories being under $1.59 and over $6.00, with increments of 50¢ between $2.00 and $3.00, and $1.00 increments above $3.00. Nearly half the subjects expected to move upward by at least one wage category, as compared to pre-prison earning level, about one-half expected to stay in the same category as before, and about one-fifth expected earnings to fall.

EARNINGS PRIOR TO INCARCERATION
vs.
EARNINGS ESTIMATE MADE NEAR TIME OF RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	45 22.4%	65 32.3%	91 45.3%	201 100.0%
CONTROL	15 21.1%	22 31.0%	34 47.9%	71 100.0%
DIFFERENCE X - C =	+ 1.3%	+ 1.3%	- 2.6%	
				271

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	23.9%	32.4%	56.3%
AT OR ABOVE $3.00	12.7	31.0	43.7
TOTAL	36.6	63.4	(+19.7)
EXPERIMENTAL			
BELOW $3.00	25.4%	24.9%	50.2%
AT OR ABOVE $3.00	10.0	39.8	49.8
TOTAL	35.3	64.7	(+14.9)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	+ 1.5%	- 7.5%	- 6.1%
AT OR ABOVE $3.00	- 2.7	+ 8.8	+ 6.1
TOTAL	- 1.3	+ 1.3	(- 4.8)

EARNINGS PRIOR TO INCARCERATION

vs.

EARNINGS IN FIRST MONTH FOLLOWING RELEASE
(177 Cases)

This comparison involves nearly 100 fewer cases than the one just preceding. In this sample, controls had been slightly (3 percentage points) more successful than experimentals in cracking the $3.00/hour barrier prior to imprisonment. They were, however, 9 percentage points less successful in cracking it after release, so the net relative change favored experimentals by 12 percentage points. In both the experimental and control samples, fewer subjects reached $3.00 than had formerly held it, but the drop was only 2 percentage points among experimentals, and nearly 15 percentage points among controls.

Twenty-six percent of experimentals and 29% of controls stayed in an identical earning category for the pre-prison vs. one month follow-up comparison; 46% of controls and 39% of experimentals fell at least one category; 43% of experimentals and 24% of controls climbed at least one earning category. Despite an 18 percentage point advantage of experimentals over controls in earnings improvement between pre- and post-incarceration, chi-square test of the full 2 x 3 table (experimental or control by decline, constancy, or rise) fails to yield a statistically significant finding ($X^2 = 3.33$; 2 df, $p < .25$). Collapse of the table, with decline and constancy combined would just meet the trend significance threshold ($X^2 = 2.70$; $p < .10$), but, in the absence of prior knowledge about distribution, the more legitimate collapse would involve combination of constancy and rise (median split convention), and test of the resulting table would not approach statistical significance. It is also necessary to note that only persons with some follow-up earnings to report are included in such tables, and that unemployment was higher among experimentals than controls in the first month after release.

TABLE 16

EARNINGS PRIOR TO INCARCERATION

vs.

EARNINGS IN FIRST MONTH FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	53 39.0%	35 25.7%	58 42.6%	136 100.0%
CONTROL	19 46.3%	12 29.3%	10 24.4%	41 100.0%
DIFFERENCE X - C =	- 7.3%	- 3.6%	+18.2%	
				177

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER: BELOW $3.00	LATER: AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	31.7%	14.6%	46.3%
AT OR ABOVE $3.00	29.3	24.4	53.7
TOTAL	61.0	39.0	(-14.7)
EXPERIMENTAL			
BELOW $3.00	29.4%	19.9%	49.3%
AT OR ABOVE $3.00	22.1	28.7	50.7
TOTAL	51.5	48.5	(- 2.2)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	- 2.3%	+ 5.3%	+ 3.0%
AT OR ABOVE $3.00	- 7.2	+ 4.3	- 3.0
TOTAL	- 9.5	+ 9.5	(+12.5)

EARNINGS ESTIMATE NEAR TIME OF RELEASE

vs.

EARNINGS IN FIRST MONTH FOLLOWING RELEASE
(190 Cases)

Controls were 9 percentage points more likely than experimentals to expect they would cross the $3.00 threshold within six months after release; for the first month following release they were 3 percentage points less successful in actually crossing it. Eighteen percent fewer experimentals, and 30% fewer controls had attained $3.00/hour than aspired to reach that level before six months would pass.

For specific earning categories, 21% of experimentals and 15% of controls had already exceeded the mark they had set for themselves, 47% of experimentals and 40% of controls had not yet managed to reach their mark, and 32% of experimentals and 45% of controls were in the earning category they had specified they were likely to hit during their first six months in the community. Somewhat paradoxically, then we find that 12% more controls are accurate in "predicting" specific level of earnings, but that 12% fewer are accurate in predicting they would reach the $3.00/hour earning level. This finding is partly attributable to more errors made by experimentals who overmet their expectation; it is again necessary to note that cases who expected no earnings and cases who attained no earnings, regardless of expectation, are excluded from this analysis.

TABLE 17

EARNINGS ESTIMATE NEAR TIME OF RELEASE

vs.

EARNINGS IN FIRST MONTH FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	67 46.9%	46 32.2%	30 21.0%	143 100.0%
CONTROL	19 40.4%	21 44.7%	7 14.9%	47 100.0%
DIFFERENCE X - C =	+ 6.5%	-12.5%	+ 6.1%	190

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

CONTROL

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	27.7%	2.1%	29.8%
AT OR ABOVE $3.00	31.9	38.3	70.2
TOTAL	59.6	40.4	(-29.8)

EXPERIMENTAL

BELOW $3.00	30.8%	7.7%	38.5%
AT OR ABOVE $3.00	25.9	35.7	61.5
TOTAL	56.6	43.3	(-18.2)

DIFFERENCE: EXPERIMENTAL—CONTROL

BELOW $3.00	+ 3.1%	+ 5.6%	+ 8.7%
AT OR ABOVE $3.00	- 6.0	- 2.6	- 8.7
TOTAL	- 3.0	+ 2.9	(+11.6)

EARNINGS PRIOR TO INCARCERATION

vs.

EARNINGS WITHIN THREE AND SIX MONTHS FOLLOWING RELEASE

(200 Cases; 185 Cases)

The pattern of relationships between pre-prison earning level and follow-up earning level after three and six months is quite similar to that for one month follow-up. Controls continued to show a deficit, though its size diminished (to 8 percentage points), on the comparison of how many claimed \$3.00/hour preceding prison vs. how many were to attain that level after release. In contrast, 5% to 6% more experimentals were successful in reaching that level than had reached it prior to incarceration. The net differences in change between experimentals and controls at three and six months follow-up are more attributable to controls formerly over \$3.00/hour falling through the barrier, than to experimentals formerly beneath it climbing through it.

For specific earning categories, there is again the same pattern of movement, and similar magnitudes of change, as were found on the one month follow-up, with experimentals continuing to be more likely than controls to improve their earnings level, and less likely than controls to merely stay at, or to fall beneath an earning level they had reached preceding their incarceration. Statistical tests of 2 x 3 tables provide the same results as before -- an advantage favoring experimentals, but one expected to occur by chance alone. Collapsing the tables to 2 x 2 with earnings decline and constancy combined continues to reach trend level significance (3 months: $X^2 = 2.73$; $p < .10$; 6 months: $X^2 = 2.99$; $p < .10$), and this collapse gains some legitimacy by virtue of a shift in median. The findings are, of course, not truly independent of one another, but have some built-in redundancy.

EARNINGS PRIOR TO INCARCERATION

vs.

EARNINGS WITHIN THREE MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	50 32.7%	38 24.8%	65 42.5%	153 100.0%
CONTROL	20 42.6%	14 29.8%	13 27.7%	47 100.0%
DIFFERENCE X − C =	− 9.9%	− 5.0%	+14.8%	
				200

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	25.5%	21.3%	46.8%
AT OR ABOVE $3.00	29.8	23.4	53.2
TOTAL	55.3	44.7	(− 8.5)
EXPERIMENTAL			
BELOW $3.00	25.5%	24.2%	49.7%
AT OR ABOVE $3.00	18.3	32.0	50.3
TOTAL	43.8	56.2	(+ 5.9)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	0.0%	+ 2.9%	+ 2.9%
AT OR ABOVE $3.00	−11.5	+ 8.6	− 2.9
TOTAL	−11.5	+11.5	(+14.4)

TABLE 19

EARNINGS PRIOR TO INCARCERATION

vs.

EARNINGS WITHIN SIX MONTHS FOLLOWING RELEASE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	48 32.9%	36 24.6%	62 42.5%	146 100.0%
CONTROL	18 46.2%	11 28.2%	10 25.6%	39 100.0%
DIFFERENCE X – C =	+13.3%	– 3.6%	+16.9%	
				185

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

CONTROL

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	25.6%	20.5%	46.2%
AT OR ABOVE $3.00	28.2	25.6	53.8
TOTAL	53.8	46.2	(– 7.6)

EXPERIMENTAL

EARLIER	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	21.9%	26.7%	48.6%
AT OR ABOVE $3.00	21.9	29.5	51.4
TOTAL	43.8	56.2	(+ 4.8)

DIFFERENCE: EXPERIMENTAL–CONTROL

EARLIER	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	– 3.7%	+ 6.2%	+ 2.4%
AT OR ABOVE $3.00	– 6.3	+ 3.9	– 2.4
TOTAL	–10.0	+10.0	(+12.4)

EARNINGS ESTIMATE NEAR TIME OF RELEASE

vs.

EARNINGS WITHIN THREE AND SIX MONTHS FOLLOWING RELEASE

(217 Cases; 200 Cases)

Patterns of relationship between earnings anticipated and earning levels attained by the end of three and six months after release are less similar to one another, and to those between anticipation and first month earnings, than the relationships between pre-prison and follow-up earnings.

For comparisons involving the \$3.00/hour threshold, there are two types of congruence -- low-low and high-high -- between expectation and actuality, and two types of non-congruence -- low-high and high-low. The category of low expectation and low realization held 25-30% of the members of both control and experimental samples at each period of follow-up. Low expectation-high realization cases never exceeded 5% of the control membership, but grew from holding 8% of experimentals at one month to containing 13% at six months. High expectation-low realization cases diminished from 32% of controls at one month to 24% at six months; there were proportionately fewer high-low cases at one month (26%) among experimentals, and nearly the same number after six months (23%). High-high congruent cases rose from 40% of controls at one month to 50% at six months, and a parallel rise from 43% to 52% occurred for experimentals. The net effect of these changes was unequal expectations of earnings above \$3.00/hour between controls and experimentals resulting in near equal levels of actual attainment.

For the specific earning category comparisons, accurate predictions of actual earning level at six months follow-up was substantially greater for controls than experimentals; 50% of the former "hit their mark," compared to 28% of the latter - a 22 percentage point difference in accuracy. Prediction errors of pessimism were quite disparate -- only 12% of control cases made them, compared to 31% of experimentals who achieved a higher hourly wage than they had expected. Test of the full 2 x 3 table yields a statistically significant finding ($X^2 = 9.14$, 2 df; $p < .025$) confirming that experimentals were fortunately more prone to errors of pessimism than controls, although both were equally prone, and more so, to errors of optimism. The conclusion is subject to the usual qualification -- that it is applicable only for cases with some earnings to report. It should also be noted that the priorities for answering the item were current wage if employed and best wage if unemployed. Thus, some individuals could continue to score high on the earnings measure even if they lost the job at an early point and remained unemployed thereafter, while a similar case who instead moved to and stayed on a lower paying job would be counted at the lower level at later points.

TABLE 20

EARNINGS ESTIMATE NEAR TIME OF RELEASE

vs.

EARNINGS WITHIN THREE MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	63 38.2%	52 31.5%	50 30.3%	165 100.0%
CONTROL	23 44.2%	19 36.5%	10 19.2%	52 100.0%
DIFFERENCE X - C =	- 6.0%	- 5.0%	+11.1%	217

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER BELOW $3.00	LATER AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	26.9%	3.8%	30.8%
AT OR ABOVE $3.00	28.8	40.4	69.2
TOTAL	55.8	44.2	(-25.0)
EXPERIMENTAL			
BELOW $3.00	27.9%	10.9%	38.8%
AT OR ABOVE $3.00	19.4	41.8	61.2
TOTAL	47.3	52.7	(- 8.5)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	+ 1.0%	+ 7.1%	+ 8.0%
AT OR ABOVE $3.00	- 9.4	+ 1.4	- 8.0
TOTAL	- 8.5	+ 8.5	(+16.5)

TABLE 21

EARNINGS ESTIMATE NEAR TIME OF RELEASE

vs.

EARNINGS WITHIN SIX MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	64 40.5%	45 28.5%	49 31.0%	158 100.0%
CONTROL	16 38.1%	21 50.0%	5 11.9%	42 100.0%
DIFFERENCE X - C =	+ 2.4%	-21.5%	+19.1%	
				200

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER: BELOW $3.00	LATER: AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	26.2%	4.8%	31.0%
AT OR ABOVE $3.00	23.8	45.2	69.0
TOTAL	50.0	50.0	(-19.0)
EXPERIMENTAL			
BELOW $3.00	24.7%	13.3%	38.0%
AT OR ABOVE $3.00	22.8	39.2	62.0
TOTAL	47.5	52.5	(- 9.5)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	- 1.5%	+ 8.5%	+ 7.0%
AT OR ABOVE $3.00	- 1.0	- 6.0	- 7.0
TOTAL	- 2.5	+ 2.5	(+ 9.5)

EARNINGS WITHIN ONE MONTH FOLLOWING RELEASE

vs.

EARNINGS WITHIN THREE AND SIX MONTHS FOLLOWING RELEASE

(193 Cases; 169 Cases)

Using the $3.00/hour threshold measure, earning level shows high stability between one and three month release exposure periods, with 90% of controls and 87% of experimentals staying in congruent categories -- either below $3.00 at both points in time or at or above it at both points. Eight percent of controls and 9% of experimentals climbed through the $3.00 barrier between month one and month three, while 2% of controls and 4% of experimentals fell through that barrier in the same interim. Despite the slight relative loss, experimentals retained a slight net edge over controls in terms of the proportion earning $3.00/hour or more, and both samples slightly increased the number of cases above that threshold.

For specific earning categories, slightly fewer experimentals than controls remained at the same earning level, with very slightly more moving downward in earnings and slightly more also moving upwardcin earnings.

In the comparison of one month with six month follow-up period, there are slightly more experimentals that controls who stay above the $3.00 per hour barrier, slightly more controls who stay beneath it, slightly more experimentals who fall through the barrier, and slightly more controls who climb through it. The net effect remains a slightly greater increase for controls at or above $3.00/hour, but a slight relative advantage for experimentals still remaining.

Comparison for one and six months on earnings category shows 59% of experimentals and 63% of controls staying in the same category both times, 31% of experimentals and 24% of controls improving their wage by at least one category step, and 10% of experimentals and 12% of controls reporting reduced hourly earnings.

Separate comparison of three month earning level against six month earning level yields the same general picture as comparison of either with one month earning level. The pattern among all is one of moderate improvement over time, with gains for some members offset to some extent by losses for other members, and relative differences between experimentals and controls showing little change over time, and never being very large in magnitude.

TABLE 22

EARNINGS WITHIN ONE MONTH FOLLOWING RELEASE

vs.

EARNINGS WITHIN THREE MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	11	105	27	143
	7.7%	73.4%	18.9%	100.0%
CONTROL	3	40	7	50
	6.0%	80.0%	14.0%	100.0%
DIFFERENCE X - C =	1.7%	6.6%	4.9%	
				193

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER BELOW $3.00	AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	50.0%	8.0%	58.0%
AT OR ABOVE $3.00	2.0	40.0	42.0
TOTAL	52.0%	48.0%	(+ 6.0%)
EXPERIMENTAL			
BELOW $3.00	44.1%	9.1%	53.1%
AT OR ABOVE $3.00	4.2	42.7	46.9
TOTAL	48.2	51.7	(+ 4.8)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	- 5.9%	+ 1.1%	- 4.9%
AT OR ABOVE $3.00	+ 2.2	+ 2.7	+ 4.9
TOTAL	- 3.8	+ 3.7	(- 1.2)

TABLE 23

EARNINGS WITHIN ONE MONTH FOLLOWING RELEASE

vs.

EARNINGS WITHIN SIX MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	13 10.2%	75 58.6%	40 31.2%	128 100.0%
CONTROL	5 12.2%	26 63.4%	10 24.4%	41 100.0%
DIFFERENCE X - C =	- 2.0%	- 4.8%	+ 6.8%	
				169

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
CONTROL			
BELOW $3.00	46.3%	14.6%	61.0%
AT OR ABOVE $3.00	4.9	34.1	39.0
TOTAL	51.2	48.8	+ 9.8
EXPERIMENTAL			
BELOW $3.00	43.0%	12.5%	55.5%
AT OR ABOVE $3.00	7.0	37.5	44.5
TOTAL	50.0	50.0	(+ 5.5)
DIFFERENCE: EXPERIMENTAL-CONTROL			
BELOW $3.00	- 3.3%	- 2.1%	- 5.5%
AT OR ABOVE $3.00	+ 2.1	+ 3.4	+ 5.5
TOTAL	- 1.2	+ 1.2	(- 4.3)

TABLE 24

EARNINGS WITHIN THREE MONTHS FOLLOWING RELEASE

vs.

EARNINGS WITHIN SIX MONTHS FOLLOWING RELEASE

DIRECTION OF INDIVIDUAL CHANGE

	DOWN	EQUAL	UP	TOTAL
EXPERIMENTAL	14	112	31	157
	8.9%	71.3%	19.7%	100.0%
CONTROL	4	37	7	48
	8.3%	77.1%	14.6%	100.0%
DIFFERENCE X - C =	+ 0.6%	- 5.8%	+ 5.1%	
				205

INDIVIDUAL CHANGE RELATIVE TO $3.00/HR. THRESHOLD

CONTROL

EARLIER	LATER		
	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	45.8%	8.3%	54.2%
AT OR ABOVE $3.00	4.2	41.7	45.8
TOTAL	50.0	50.0	(+ 4.2)

EXPERIMENTAL

EARLIER	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	40.1%	8.9%	49.0%
AT OR ABOVE $3.00	5.7	45.2	51.0
TOTAL	45.9	54.1	(+ 3.1)

DIFFERENCE: EXPERIMENTAL-CONTROL

EARLIER	BELOW $3.00	AT OR ABOVE $3.00	TOTAL
BELOW $3.00	- 5.7%	+ 0.6%	- 5.2%
AT OR ABOVE $3.00	+ 1.5	+ 3.5	+ 5.2
TOTAL	- 4.1	+ 4.1	- 1.1

OCCUPATIONAL CATEGORIES AND CAREER CHANGE

WISCONSIN

The Dictionary of Occupational Titles groups occupations into nine broad categories, which, in turn, are divided into divisions and then groups. The broad occupational categories are listed below, ordered by the frequency in which job titles tended generally to be found among members of the Wisconsin study sample at various comparison points.

RANK	GROUP
1st	Structural Work Occupations
2nd	Miscellaneous Occupations
3rd	Machine Trades Occupations
4th	Service Occupations
5th	Processing Occupations
6th	Professional, Technical, and Managerial Occupations
7th	Bench Work Occupations
8th	Clerical and Sales Occupations
9th	Farming, Fishery, Forestry, and Related Occupations

The above order was generated by simple summation of each occupation category's rank position in twelve orders -- a separate order for controls, and for experimentals on:

N = 181 a. Highest paying job preceding incarceration.
N = 149 b. Best job expected after release at time of intake.
N = 180 c. Best job expected after release at time of release.
N = 122 d. Highest paying job held within 1 month after release.
N = 138 e. Highest paying job held within 3 months after release
N = 127 f. Highest paying job held within 6 months after release.

The number of cases for whom data were coded was reduced to the above, from a full 200, by a number of factors -- data not due or not supplied or not applicable (i.e., no job) or not codable (i.e., not described adequately).

Spearman's rank order correlation (with correction for ties applied) was run between the summative order and each of the twelve separate orders. Rank position of occupational categories, in terms of the relative frequency of subjects whose job fell in each category, was quite stable, with rho never dropping below .883 on the six orders involving job after release (with 1.000 representing perfect correlation, .746 necessary to reach the $p < .01$ significance level, one-tailed test, and .564 for $p < .05$) and dropping below .820 only for the control sample's best job preceding intake (rho = .675) and the control sample's expectation at time of release (rho = .644).

The top category -- *structural work* occupations -- held between 22% and 28% of subjects at each point of inquiry, and the bottom category -- *farming and related* -- held no more than 3% at any point of inquiry. Fluctuation was greater for other categories -- particularly for the *professional, technical, managerial* category, which never contained more than 11% on actual job, nor as few as 11% on expected job.

Detailed comparison of occupational expectations at time of release and actual occupations after six months follow-up indicates that a lower proportion of both experimentals and controls were to find themselves in the *professional, technical, managerial* category than had expected to be there, and that a higher proportion than expected found placements in the *miscellaneous* category, most often as laborers. For experimentals, actual placement proportions were also beneath the anticipated level in all other occupational categories except *processing* occupations, and *bench work* occupations. For controls, placement proportion was beneath expection for only one other occupational category -- *bench work* occupations, were exactly at the anticipated level for *structural* occupations, for *processing* occupations, and for *farm and related* occupations, and were proportionately higher than anticipated in the *machine trades*, the *services*, and the *clerical and sales* occupation categories. Differences between the distributions of expected and actual placement proportions were ordinarily slight, exceeding 5 percentage points only for the *professional, managerial, and technical*, and for the *service* categories among both controls and experimentals, and also this disparate among only experimentals for the *miscellaneous* category.

Detailed comparison of the distribution of occupations held prior to incarceration and six months subsequent to release from prison indicates that, for experimentals only one occupation category showed either an increase or decrease of greater than 5 percentage points -- *bench trades* accounted for only 3% of subjects' occupations before imprisonment, and for 11% after imprisonment; slight increases for experimentals were also shown in the *machine trades*, and the *miscellaneous* categories. Among controls, increases greater than 5 percentage points between pre-prison and post-prison occupations were found for machine trades, and for service categories, each accounting for 7% of controls prior to imprisonment, and for 17% subsequent to imprisonment, and these were the only two categories in which any increase at all was established -- all others fell, but only the professional, technical, and managerial by more than five percentage points.

Disparities in the distribution of occupations for experimentals and controls were greatest, prior to incarceration, in two categories -- clerical and processing, which were more frequent occupations among controls, and these disparities were affected by less than one percentage point subsequent to release. Disparity shrank by as much as 3 percentage points only for the machine trades category, for which the direction of disparity was also reversed, with controls establishing a higher proportion of membership relative to experimentals. Disparity between experimentals and controls increased most markedly in the miscellaneous category, with direction of disparity again showing reversal -- a 14 percentage point shift moving experimentals from 4% beneath controls to 10% above them. There were two other categories in which disparity increased by more than 5 percentage points -- bench trades, in which experimentals established a 10% lead over controls during follow-up, and services where controls established a 6% lead. The distributions of subjects among occupational categories on six month follow-up for experimentals and controls are fairly similar, and the rank order correlation between them is .704; it had been .685 preceding imprisonment. The correlation of occupational order among experimentals for pre- and post-imprisonment was .818; for controls it was .701. Only one occupation category showed a displacement of more than three positions in rank -- bench trades, which moved from 8th place to 4th for experimentals.

A combination of the structural and the miscellaneous (primarily laborer) categories generally accounted for two-fifths to one-half of Wisconsin workers -- both pre- and post-imprisonment, and both experimental and control.

TABLE 25

OCCUPATIONAL CATEGORY DISTRIBUTION FOR HIGHEST PAYING JOB PRECEDING INCARCERATION AND HIGHEST PAYING JOB REPORTED AFTER SIX MONTHS FOLLOW-UP

OCCUPATIONAL CATEGORY	EXPERIMENTAL			CONTROL			EXPERIMENTAL-CONTROL		
	PRE	POST	DIFF	PRE	POST	DIFF	PRE DIFF	POST DIFF	DIFF IN DIFFS*
STRUCTURAL WORK	22.7%	21.6%	- 0.1%	27.9%	26.7%	- 1.2%	- 5.2%	- 5.1%	- 0.1%
MISCELLANEOUS	22.7	26.8	+ 4.1	18.6	16.7	- 2.3	- 4.1	+10.1	+14.2
MACHINE TRADES	12.3	14.4	+ 2.1	7.0	16.7	+ 9.7	+ 5.3	- 2.3	+ 7.6
SERVICES	10.9	9.2	- 1.7	7.0	16.7	+ 9.7	- 1.7	- 7.5	+ 5.8
PROCESSING	9.4	7.2	- 2.2	16.3	13.3	- 3.0	- 6.9	- 6.1	- 0.8
PROFESSIONAL	10.9	7.2	- 3.7	7.0	0.0	- 7.0	+ 3.9	+ 7.2	+ 3.3
BENCH WORK	3.6	11.3	+ 7.7	2.3	0.0	- 2.3	+ 1.3	+11.3	+10.0
CLERICAL & SALES	4.3	2.1	- 2.2	11.6	10.0	- 1.6	- 7.3	- 7.9	+ 0.6
FARMING	2.9	0.0	- 2.9	2.3	0.0	- 2.3	+ 0.6	- 0.0	- 0.6

*For example, the difference between experimentals and controls increased by 3.3% from pre- to post-prison job in the professional occupation category.

ARIZONA

An analysis parallel to that for Wisconsin was conducted. The summative order for distribution of occupations among members of the Arizona study sample was:

RANK	GROUP
1st	Structural Work Occupations
2nd	Service Occupations
3rd	Miscellaneous Occupations
4th	Clerical and Sales Occupations
5th	Professional, Technical, and Managerial Occupations
6th	Bench Work Occupations
7th	Machine Trades Occupations
8th	Farming, Fishery, Forestry, and Related Occupations
9th	Processing Occupations

This order differs from that for Wisconsin (the correlation between them is .533 -- a statistically non-significant degree of similarity) and the major differences between the two are the higher ranking for clerical, sales in Arizona, and the lower ranking for processing and for machine trades. Each of the twelve separate occupation orderings in Arizona (actual and anticipated, pre- and post-imprisonment, experimental and control) correlates at statistically significant levels with the summative order, but the correlations are somewhat lower than those found in Wisconsin, indicating somewhat less stability in the distribution of occupations. This may be attributable, in part, to greater data loss and less representative available samples of the full Arizona study sample -- follow-up data providing an occupational category is available for only 48% to 53% (depending on exposure period) of the full 195 member study sample.

Prior to incarceration, 23% of the best jobs claimed by both experimentals and controls were in the structural work occupation category. Among experimentals, expectation of placement in that category subsequent to release was held by 34% and, over each of the three follow-up periods, actual placements stayed between 29% and 32% of reported jobs. Forty-four percent of controls expected placement in structural work occupations, and 33%, 42%, and 50% reported having held such positions over the successive follow-up periods.

Three other occupation categories -- service, miscellaneous, and farming had each accounted for 12% -- 14%

of experimentals' reported occupations for the pre-prison period; in the post-prison period, *farming* was less frequently reported, staying between 2% and 7% for the several follow-up periods; placements in the *services* category rose to represent 15% -- 20% of employment for experimentals, and *miscellaneous* similarly rose -- to 17% -- 21%. Two of these three categories -- *services* and *miscellaneous* -- were the only ones other than *structural* to hold more than 10% of controls on reported pre-prison job, but neither category showed stability during follow-up; *services* ranged from a low of 7% at one month to a high of 17% at six months, and *miscellaneous* from a high of 33% at one month to a low of 6% at six months.

Prior to imprisonment the correlation for distribution of occupations was .624 between experimentals and controls, with the disparities primarily attributable to *farming* (higher among experimentals) and, to a lesser extent, *professional* (higher among controls). At six months subsequent to release, the correlation between occupational orders for experimentals and controls was .643 and, on this comparison, the major disparities were introduced by a higher proportion of controls in *services* and a higher proportion of experimentals in *machine trades*.

For experimentals pre- and post-prison (6 month follow-up) orders correlate .803 with no marked category disparities; for controls, pre- and post-prison orders correlate category showing a major shift in rank.

OCCUPATIONAL CATEGORY OF POST-RELEASE TRAINING DESIRED

WISCONSIN AND ARIZONA

At the time of prison release testing, members of the study sample were asked whether there were specific occupations in which they would like to obtain training after they left prison. The question had no time boundary referent and we may assume that responses would reflect long term career aspirations. In Wisconsin, 135 respondents gave replies codable into occupational category; in Arizona, 103 codable responses were obtained.

RANK		
WISCONSIN	ARIZONA	GROUP
1	1	Professional, etc.
2	2	
Tied at 3.5	4	Machine Trades
Tied at 3.5	5	Services
5	3	Bench Work
Tied at 6.5	Tied at 6.5	Clerical and Sales
Tied at 6.5	Tied at 8.5	Miscellaneous
8	Tied at 8.5	Processing
9	Tied at 6.5	Farming, etc.

The orders for the two states are similar with rho, corrected for ties, being .837, and the top two occupation categories -- professional and structural together account for about three-fifths of training choices made by subjects in each state. Correlations run between the training priority order and the summative order of occupations held or expected -- an index which was used in the preceding section, yields a positive, but statistically non-significant correlation for the Wisconsin data sets (rho = .478), and essentially no correlation for the Arizona data set (rho = .076). These findings suggest fairly widespread desire for further mobility across occupational career categories.

PRISONER AND PROGRAM
SUBGROUPS AND CRIMINAL JUSTICE SYSTEM DISPOSITIONS

PRISONER CHARACTERISTICS AND CRIMINAL JUSTICE SYSTEM DIFFICULTY FOR EXPERIMENTALS AND CONTROLS

The claim is often made for programs in corrections which appear to yield little or no favorable effect overall, that more dramatic findings were obscured because the "treatment" is not appropriate for all types of cases, but is particularly suited to some types, and wasted or even deleterious for others.

In the following section, a number of variables are examined to determine their overall relationship to arrest-free status during post-release follow-up, and a search is conducted to determine whether MAP program effects may have been concentrated within defined prisoner subgroups. The use of arrest as the criterion or disposition threshold is not so much a matter of choice as one of necessity -- use of a firmer criterion (i.e., felony conviction or return to prison) is ordinarily preferable since the implications are less trivial; given the situation of brief follow-up and rarity of serious criminal justice system disposition, "clean" vs. all other dispositions in the only option which distributes cases in any quantity to both sides of a criterion boundary.

PRISON PREPARATION FOR JOB AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

WISCONSIN AND ARIZONA DATA COMBINED

Data are available for major criminal justice system disposition at six month follow-up on a combined Wisconsin-Arizona study sample of 254 subjects, among whom 71% remained arrest-free for the full exposure period. Cross-tabulation of arrest status and the proportion of preparation subjects claimed they had obtained during their prison stay for the job they expected after release shows minor differences across the categories.

TABLE 26

AMOUNT OF PREPARATION

	PERCENT ARREST-FREE AFTER SIX MONTHS	PERCENT OF TOTAL CASES
"ALL" or "MORE THAN HALF"	74%	(33%)
"ABOUT HALF" or "LESS THAN HALF"	69%	(13%)
"VERY LITTLE" or "NONE"	68%	(46%)
NO INFORMATION	75%	(8%)
TOTAL	71%	(100%)

Separate examination of the experimental samples for evidence of relationship between claimed source of job preparation and subsequent Criminal Justice System difficulty and for differential consequences of MAP program exposure yields the following table.

TABLE 27

AMOUNT OF PRISON PREPARATION FOR JOB EXPECTED AFTER RELEASE

	"ALL" or "MORE THAN HALF"	"ABOUT HALF" or "LESS THAN HALF"	"VERY LITTLE" or "NONE"	NO INFORMATION	TOTAL
EXPERIMENTAL					
PERCENT OF CASES	(35%)	(12%)	(47%)	(6%)	(100%)
PERCENT ARREST-FREE	71%	74%	67%	92%	71%
X - C DIFFERENCE	-17%	+18%	- 7%	+42%	0
CONTROL					
PERCENT ARREST-FREE	88%	56%	74%	50%	71%
PERCENT OF CASES	(29%)	(16%)	(41%)	(14%)	(100%)

Separate tests of each column in the above table yield no statistically significant differences in arrest-free rates between experimentals and controls for any job preparation category. A slight difference (7 percentage points) exists in the most sizable category -- those claiming very little or no prison preparation, and the difference favors controls. In the next largest category -- cases claiming that all or most of the preparation for the job they expected had been obtained in prison -- a greater difference (17 percentage points) exists, with controls again more likely to remain arrest-free. These advantages are, however, offset by better arrest-free performance of experimentals in the two smaller categories -- those claiming less than half to about half of preparation obtained inside prison, and those who provided no reply to the question. Overall, the arrest-free rates are identical for experimentals and controls.

The findings provide no support for a particularized effect of MAP upon cases most dependent on prison preparations.

PERCEIVED INFLUENCE OF ECONOMIC FACTORS AND POST-RELEASE ARREST-FREE STATUS

WISCONSIN AND ARIZONA DATA COMBINED

Possible differential effects of MAP upon cases, dependant upon their beliefs about the origin of difficulties leading to their prison commitment offense were explored in the same fashion as that for prison job preparation.

Overall, cases who attributed relatively little importance to economic factors appeared to be somewhat (9 percentage points) more fortunate in remaining arrest-free than those who offered it as partial explanation of their prison commitment offense. Among controls this difference is almost non-existent, but among experimentals it reached trend significance ($X^2 = 2.74; p < .10$). However, experimentals who offer no economic rationale for offenses performed no better than either category of controls; the experimental arrest rate for those employing the rationale of low income or lack of work -- a sub-group among which we might expect MAP program effects to be concentrated -- performs less well remaining arrest-free than controls holding the same belief. The difference (12 percentage points) is not statistically significant.

TABLE 28

LOW INCOME OR LACK OF WORK CONTRIBUTED TO COMMITMENT OFFENSE

FOLLOW-UP PERFORMANCE	"A LITTLE" or "NOT AT ALL"	"A FAIR AMOUNT" or "A LOT"	NO INFORMATION	TOTAL
EXPERIMENTAL				
PERCENT OF SAMPLE	(50%)	(45%)	(5%)	(100%)
PERCENT ARREST-FREE	76%	63%	90%	71%
X - C DIFFERENCE	+ 2%	-12%	+ 50%	0
CONTROL				
PERCENT ARREST-FREE	74%	75%	40%	71%
PERCENT OF SAMPLE	(41%)	(50%)	(9%)	(100%)
TOTAL				
PERCENT ARREST-FREE	75%	66%	73%	71%
PERCENT OF SAMPLE	(48%)	(46%)	(6%)	(100%)

PRIOR IMPRISONMENT RECORD AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

WISCONSIN AND ARIZONA DATA COMBINED

The possibility of differential effects of MAP programming on release performances of first and multi-termers was explored.

Overall, subjects without prior adult imprisonment had an arrest-free rate at six months post-release exposure which was 9 percentage points than those with priors -- a difference which fails, given the sample size, to reach statistical significance. In the control sample, those without priors appear to enjoy a substantial (again, statistically non-significant) arrest-free advantage over those with priors, while, in the experimental sample, cases with a prior imprisonment record performed essentially as well as first termers, and 10 percentage points better than their multi-termers is, however, more than offset by the 11 percentage point control sample superiority among first-termers -- a numerically larger class. Neither difference reaches a statistically significant level, and the net advantage to controls is nullified by the group on which termer status was unavailable, yielding identical overall performance for experimentals and controls.

TABLE 29

TIMES IMPRISONED AS ADULT

FOLLOW-UP PERFORMANCE	ONCE	TWICE OR MORE	NO INFORMATION	TOTAL
EXPERIMENTAL				
PERCENT OF TOTAL	(62%)	(33%)	(5%)	(100%)
PERCENT ARREST-FREE AFTER SIX MONTHS	70%	67%	100%	71%
X - C DIFFERENCE	-11%	+10%	+33%	0%
CONTROL				
PERCENT ARREST-FREE AFTER SIX MONTHS	81%	57%	67%	71%
PERCENT OF TOTAL	(57%)	(38%)	(5%)	(100%)
TOTAL				
PERCENT ARREST-FREE AFTER SIX MONTHS	73%	64%	92%	71%
PERCENT OF TOTAL	(61%)	(34%)	(5%)	(100%)

ETHNIC MINORITY STATUS AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

WISCONSIN AND ARIZONA DATA COMBINED

No difference, overall, is found between members of the majority and members of minority ethnic groups on arrest-free performance after six months exposure. For the available follow-up study sample, minority membership is somewhat higher for the control than the experimental sample, and arrest-free performance for members of ethnic minority groups is 7 percentage points higher for controls. A lesser difference of 4 percentage points, with experimentals outperforming controls among the larger-sized ethnic majority sample, nullifies the other difference, neither of which is statistically significant.

TABLE 30

MEMBER OF ETHNIC MINORITY GROUP

FOLLOW-UP PERFORMANCE	YES	NO	NO INFORMATION	TOTAL
EXPERIMENTAL				
PERCENT OF SAMPLE	(40%)	(58%)	(1%)	(100%)
PERCENT ARREST-FREE	71%	70%	100%	71%
X - C DIFFERENCE	- 7%	+ 4%		0%
CONTROL				
PERCENT ARREST-FREE	78%	66%	--	71%
PERCENT OF SAMPLE	(48%)	(52%)	0%	(100%)
TOTAL				
PERCENT ARREST-FREE	70%	71%	100%	71%
PERCENT OF SAMPLE	(43%)	(36%)	(1%)	(100%)

EDUCATIONAL LEVEL AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

WISCONSIN AND ARIZONA COMBINED

Overall, subjects who had completed high school or who held GED's were 10 percentage points (though non-significantly) more likely than those with less education to remain arrest-free throughout six month follow-up. Among experimentals this difference is slightly greater (12 percentage points), and passes the threshold for trend significance ($X^2 = 2.90$; $p < .10$). Experimentals with higher levels of education were slightly more fortunate (by 6 percentage points) than higher-educated controls in staying arrest-free, but the difference is offset by slightly better (4 percentage points) arrest-free performance for controls, compared to experimentals among the numerically larger class of cases with lesser levels of education.

TABLE 31

COMPLETION OF HIGH SCHOOL OR EQUIVALENT

FOLLOW-UP PERFORMANCE	YES	NO	NO INFORMATION	TOTAL
EXPERIMENTAL				
PERCENT OF SAMPLE	(39%)	(59%)	(2%)	(100%)
PERCENT ARREST-FREE	78%	66%	67%	71%
X - C DIFFERENCE	+ 6%	- 4%	-33%	0%
CONTROL				
PERCENT ARREST-FREE	72%	70%	100%	71%
PERCENT OF SAMPLE	(32%)	(66%)	(2%)	(100%)
TOTAL				
PERCENT ARREST-FREE	77%	67%	75%	71%
PERCENT OF SAMPLE	(37%)	(61%)	(2%)	(100%)

BEST PRE-IMPRISONMENT HOURLY WAGE AND POST-RELEASE CRIMINAL JUSTICE DIFFICULTY

WISCONSIN AND ARIZONA DATA COMBINED

Overall, subjects at lesser earning levels prior to incarceration are slightly (4 percentage points) less likely to remain arrest-free for six months subsequent to release. This difference is larger among experimentals, amounting to 7 percentage point disadvantage, but for controls its direction is reversed with those subjects who had not reached the $3.00 per hour level preceding imprisonment being 4 percentage points more successful in staying arrest-free than subjects with higher post earnings, and 8 percentage points more successful than the low-earning experimental counterparts. None of the differences is statistically significant.

TABLE 32

PRE-PRISON WAGE OF $3.00/HR. OR MORE

FOLLOW-UP PERFORMANCE	YES	NO	NO INFORMATION	TOTAL
EXPERIMENTAL				
PERCENT OF SAMPLE	(42%)	(42%)	(14%)	(100%)
PERCENT ARREST-FREE	74%	67%	71%	71%
X - C DIFFERENCE	+ 3%	- 8%	+ 4%	0%
CONTROL				
PERCENT ARREST-FREE	71%	75%	67%	71%
PERCENT OF SAMPLE	(43%)	(36%)	(21%)	(100%)
TOTAL				
PERCENT ARREST-FREE	73%	69%	70%	71%
PERCENT OF SAMPLE	(43%)	(41%)	(16%)	(100%)

INTENSIVE EMPLOYMENT PLACEMENT AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

WISCONSIN

In Wisconsin, cooperative efforts were made between the Division of Corrections and the State Employment Service to provide both experimental and control cases with job placement assistance prior to release. A description of the procedures and results of the service relative to employment placement may be found in the appendix to this report. The focus in this section is, instead, upon arrest-free status for 126 subjects with both IEP and follow-up data available, released from prison through September, 1973 -- a substantial majority of all Wisconsin cases eligible for six month follow-up at the close of data collection.

Only three-fifths of subjects received any IEP service (vocational aptitude testing, counseling and training in job-finding or job-keeping, efforts to assist in arranging job interviews), with experimentals slightly more likely to do so than controls; only one-fifth obtained job interviews prior to release through IEP, and only 12% secured a position. Experimentals were slightly more likely to receive some form of IEP service, and also slightly more likely to obtain a position through IEP. Differences in arrest-free percent between persons receiving or failing to receive any service were negligible overall -- 72% for those without service, and 71% for those with service, within the experimental and control samples, taken separately, differences were also slight -- 3 percentage points higher arrested if served among controls, and 5 percentage points lower if served among experimentals.

MAP CONTRACT TERMS AND POST-RELEASE CRIMINAL JUSTICE SYSTEM DIFFICULTY

ARIZONA AND WISCONSIN DATA COMBINED

MAP contracts signed by experimentals were coded on a number of variables, and many of the categories employed yield insufficient frequencies to permit meaningful comparison. At a gross level one type of coding dealt merely with whether some activity in six broad areas was included or deleted as a contract term.

One hundred and thirty-three of the 254 cases available for six month follow-up were contract experimentals. The overall arrest-free percent for contract experimentals was 67%, versus 78% for non-contract experimentals and 71% for controls.

The following comparisons deal only with contract cases, showing the percent whose contract contained a given term, and the arrest-free rates for those with and without such a term.

TABLE 33

CONTRACT TERM	CONTRACTS CONTAINING TERM	ARREST-FREE PERCENT AFTER 6 MO.		DIFF
		WITH TERM	WITHOUT TERM	
1. SKILL TRAINING	62%	68%	65%	+ 3%
2. EDUCATION	62%	70%	61%	+ 9%
3. JOB ASSIGNMENT	48%	73%	61%	+12%
4. TREATMENT	82%	68%	61%	+ 7%
5. DISCIPLINE	80%	67%	64%	+ 3%

Terms involving attendance at counseling or treatment sessions and those requiring some specified avoidance of disciplinary infractions, were the most frequent elements of MAP contracts, appearing in four-fifths of all contracts. Vocational skill training or general education terms each appeared in three-fifths of the contracts, and prison work assignments in about half. In each of the five contract term areas, presence of some requirement was associated with a slightly higher arrest-free standing after six months follow-up, but only in the case of terms involving work assignment performance does the association reach a trend level significance threshold ($X^2 = 3.23$; $p < .10$). To some extent, the arrest-free differences involving presence or absence of a contract term could be a by-product of prior status characteristics of prisoners, but the performance differences are not of a size warranting further exploration of source, and the small sample sizes involved would make it unlikely that further clarity would be attained.

Provision had existed to record difficulties arising in the course of contract implementation in any of the five broad term areas. Few such difficulties were recorded, and nearly all these involved disciplinary infractions. Twelve contract cases survived a disciplinary infraction without cancellation of their contract and, of these, nine, or 75% remained arrest-free during the first six months following release, and the most serious disposition received by the other three was in the Uniform Parole Report "minor" category, usually counted as favorable outcome.

Work assignment contract terms were ordinarily phrased to describe the prison job and period of work expected of the prisoner, but about a third of cases with work assignment terms also had some general performance stipulation -- usually "maintain at least average or satisfactory work reports." The arrest-free percent at six months for the small sample of cases with such stipulations was 58%

A majority of the education terms found in contracts dealt with provisions to study for and to take or pass a GED equivalency test. Fifty-three percent of cases with these contract stipulations completed six arrest-free months following release, compared to 73% of contract cases with no such stipulation. The difference is statistically significant ($X^2 = 4.08$; $p < .05$). Treatment terms of contracts, in 90% of the cases which included such terms, stipulated "counseling," usually group, and ordinarily making further specifications about duration of involvement and/or frequency of sessions. For cases in which counseling was specified rather than provision for "therapy," or a combination was to occur, or the type was left open, the arrest-free percent was 63% at six months, compared to 89% for the few cases involving other treatment options.

In comparisons of the type made above, performance differences among subgroups, even if found, defy unambiguous interpretation because program effects may be seriously contaminated by selection effects, and efforts to introduce control through matching subgroups on other characteristics results in serious damage to available sample sizes. In the present circumstances there are other reasons, as well, to limit inquiry along these lines and accept that there was no program effect, rather than continue partitioning the sample in a search for buried effects. On the question, for example, of whether a similar approach should be applied to an employment criterion rather than a recidivism criterion, it is pertinent first to note that fewer than falf the contracts contained a term related to prison job assignment. Further examination reveals that a high proportion of those job assignment terms which were written (about two-thirds, in fact) stipulated merely that the prisoner would continue on whatever job assignment he held before entering the contract, and that less than a fifth of such terms mentioned any necessary level of performance (eg. "maintain at least average or satisfactory work reports.") Similarly, with regard to skill training terms, which appeared in about three-fifths of the contracts written, formalization of pre-existing program arrangements appears to have been more frequent than involvement in fresh commitments, and where the latter are found they were often hedged with conditions that lessened their binding quality. In order to more tangibly illustrate these points, verbatim transcripts of the entire skill training term is provided below for a 20% sample of all contracts completed in each state. Cases were selected by arranging them in order of ascending ID#, and including every fifth case in the sample.

ARIZONA

#012	(No skill training term. 5 month contract.)
#025	(No skill training term. 4 month contract.)
#033	"Received barber training in the Illinois State Prison. OJT outside trusty barber shor." (4 month contract.)
#042	"Will successfully complete Vocational Welding 12/22/72." (Contract signed 12/18/72 for six month period.)
#056	"Will successfully complete Vocational Upholstery 12/22/72." (Contract signed 12/13/72 for one month period.)
#069	"Will successfully complete Vocational Welding 6/73." (Contract signed 12/18/72 for seven month period.)
#091	(No skill training term. 6 month contract.)
#103	(No skill training term. 2 month contract.)
#128	(No skill training term. 2 month contract.)
#149	"Successfully completed Vocational Welding 12/22/73." (Contract signed 1/11/73 for 3 month period.)
#161	"Agree to successfully complete Vocational Auto Body and Fender Program by 6/73." (Contract signed 4/10/73 for three month period.)
#174	"Agree to successfully complete Vocational TV and Radio Repair by 6/73." (Contract signed 4/16/73 for two month period.)

#192 (No skill training term. 4 month contract.)

#205 "Successfully completed Vocational Welding 12/22/72."
(Contract signed 1/11/73 for seven month period.)

It can be noted, above, that in Arizona the skill training condition was often on the verge of completion (and sometimes already completed) at the point of contract entry, and yet that the contractual release date was usually set some months away in violation of the MAP tenet that release should coincide with completion of training. Further, it is evident that a prisoner's prior success in arranging enrollment in a formal training course was quite instrumental in the parole board's decision to permit contract entry -- examination of the cases with contracts denied or withdrawn by the board reveals that over two-thirds had a blank skill training term.

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#023 (No skill training term. 5 month contract.)

#034 "Has taken General Aptitude Test Battery and started Manual Skills course 12/26/72. Will continue in Manual Skills and will meet expectations of instructor. Will continue in the course until the instructor feels I have reached maximum benefit."
(Contract signed 1/4/73 for seven month period.)

#049 (No skill training term. 4 month contract.)

#060 (No skill training term. 3 month contract.)

#071 (No skill training term. 5 month contract.)

#086 "Will take General Aptitude Test Battery and will enroll in a manual skills area if I am found capable in one of the training areas."
(Contract signed for eight month period.)

#096 "Complete Basic Welding and complete present phase of blueprint reading."
(Contract signed for nine month period.)

#121 "If approved through regular channels, will enroll in the truck drivers training class at Fox Valley Technical School. If the class is obtained, will complete prior to release on parole."
(Contract signed for seven month period.)

#136 No skill training term. 9 month contract.)

#157 "Will enroll in Masonry and Blueprint Reading and will complete the goals and expectation as determined by the teacher."
(Contract signed for seven month period.)

#172 "I am presently enrolled in Auto Body and will satisfactorily complete Auto Body Repair I course and satisfactorily complete Auto Body Repair II course. Upon completion of the above, I will remain in Auto Body with the concurrence of the instructor.
(Contract signed for seven month period.)

#184 (No skill training term. 4 month contract.)

#194 "Will explore Small Engine training class. If course content is appropriate, will apply for admission to the course. Upon admission, will become involved in the class."
(Contract signed for four month period.)

As compared with Arizona, skill training terms in Wisconsin appear more future-oriented, subject to more contingencies, and without any ready reference point linking training completion to release date. (Although the standardized formal wording of contracts in both Arizona and Wisconsin provided for release contingent upon the prisoner's successful completion of objectives "on or before" the specified contract termination date, no case in either state had his release accelerated to occur before the originally agreed date.) Absence of a skill training term in a Wisconsin prisoner's proposed contract did not appear to jeopardize his chances as it would in Arizona -- contracts signed in Wisconsin were as likely to contain no such term as were contracts denied by the parole board.

Given the nature of the training terms written into MAP contracts, and that the fact of prison release on the specified date, rather than any separate and independent inquiry, was accepted as evidence that the contractual terms had in fact been met, further analyses of post-prison performance differences among subgroups with differing contract terms (eg. those training in bench trades vs. those training in structural work occupations) would seem, on their face, to tell us less about the effects of contract programming than one might otherwise expect, and there is consequently less reason to lament that sample sizes are too small to permit such comparison.

COMPARISONS AFTER RELEASE

SUMMARY

EXPERIMENTALS VS. CONTROLS

On over fifty comparisons involving post-release variables, one might expect chance alone to generate about five differences statistically significant at $p<.10$, three at $p<.05$, and one at $p<.025$. The actual results are generally within that chance expectancy range. There were, in fact, two differences yielding significance at $p<.025$, showing experimentals (combined state samples) more likely than controls at time of release to underestimate the level of hourly earnings they would later actually manage to attain within three and within six months subsequent to release, though both experimentals and controls were more likely to err through overestimation. There were, in fact, two differences found significant at $p<.10$, involving comparison of hourly earnings prior to imprisonment with those at three and six months follow-up after release; experimentals (again, combined state samples), for both comparison periods, were more likely than controls to experience some improvement in hourly earnings. Little confidence, however, can be placed in the reliability of these findings, given the fact that chance would bestow a few such findings whenever numerous comparisons are made.

Apart from the three findings above, all based on measures of individual change over time, there were only two more, involving aggregate differences, each significant at $p<.01$, and found for the same variable in both Arizona and Wisconsin: After six months in the community, all members of the available study sample were to be asked, "Did MAP help you in any way at all?" Responses were obtained for about 80% of cases, and experimentals were significantly more likely than controls to answer, "Yes," (Among contract experimentals, 78% in Wisconsin and 66% in Arizona agreed, reversing the pattern found on responses prior to release, when Arizona subjects generally found MAP more praiseworthy than Wisconsin subjects did. Twenty-eight percent of Wisconsin controls, and 12% of Arizona controls claimed, six months after release, that they had somehow been helped by MAP.)

Examination simply of the direction, rather than magnitude of post-release differences favoring experimentals or controls on several aggregate measures with most direct bearing on the topics mentioned in the grant proposals for this project (see Preface), reveals:

1. Arrest-free status: Controls ahead on 5 of 6 occasions. (Exception -- Arizona for 6 month follow-up.)

2. Returns to prison: Controls ahead on 4 of 6 comparisons. (Exception -- Tied with experimentals at 0 returns at one month for both states.)

3. Time until first job: Controls ahead on 2 of 2 occasions.

4. Full-time employment: Controls ahead on 5 of 6 comparisons. (Exception -- Arizona for 6 month follow-up.)

5. Job retention: Controls ahead on 4 of 4 comparisons.

6. Job-relevant prison training: Controls ahead on 3 of 6 comparisons. (Exception -- Arizona for 1 month, Wisconsin for 3 and 6 month follow-up.)

7. Training after release: Controls ahead on 4 of 6 comparisons. (Exception -- Arizona for 1 and 6 month follow-up.)

8. Earning level: Experimentals ahead on 5 of 6 comparisons. (Exception -- Arizona for 1 month follow-up.)

In the absence of evidence of program effects from examination of aggregate differences, or from individual change measures, a third line of search, involving partitioning of the sample in order to explore the possibility of differing program impact on various offender subclasses, was conducted. Were, for example, positive MAP effects on some identifiable subclasses being offset by negative effects on others, or simply obscured through a general lack of effect among others?

With arrest-free status after six months follow-up set as the criterion variable, and no overall difference between control and experimentals on that measure, the basic comparison groups were subdivided on education level, pre-prison earnings, ethnic status, prior imprisonment, claimed prison preparation for a job, and belief that economic factors were causative in their crime. Again, no statistically reliable differences were to be found, and the direction of obtained differences was often contrary to that one would posit. For example:

EXPERIMENTALS OUTPERFORMED CONTROLS WHEN:	BY:	CONTROLS OUTPERFORMED EXPERIMENTALS WHEN:	BY:
pre-prison wage at or above $3.00/hour.	(3%)	Pre-prison wage below $3.00/hour	(8%)
High school completed	(6%)	High school not comp.	(4%)
Not minority ethnic	(4%)	Minority ethnic group	(7%)
Does not attribute offense to low income	(2%)	Blames low income for offense	(12%)
Half or less job preparation in prison	(18%)	Most or all job preparation in prison	(17%)
Imprisoned more than once	(11%)	Imprisoned only once	(10%)

With four or five of the six variables (prior imprisonment is an exception and prison preparation is ambiguous) the difference favoring experimentals over controls on arrest-free percent is among the members of what one might assume is the more priviledged social category, whereas differences favoring controls occur among members of the less privileged category. Given that the differences are not large, and that the involved sample sizes are relatively small, it would be premature to conclude that MAP operates to increase disparity in social advantage (i.e., "them that has gets"), but such a possibility warrants continued scrutiny.

CONTRACT VS. NON-CONTRACT

Only four differences reaching the $p < .10$ trend threshold were found despite comparison on numerous variables, and each of the four just reached that level. Since these were fully likely to have been generated within the realm of chance, we shall not discuss them here, but merely conclude that no significant differences exist between contract and non-contract experimentals on post-release measures.

MAP IN CALIFORNIA

The history of implementation of Mutual Agreement Programming in California through the Parole-Corrections Project is one filled with repeated disaster and occasional triumph. The bulk of material in the research files on the California experience consists of meeting notes and memoranda -- mostly dealing with alteration of program and research designs, and interruptions in implementation. The following meeting notes are illustrative, and concern a meeting held nearly a year after tentative agreements were reached for California involvement, and seven months after a memorandum (dated November 30, 1973) to the project director from the administrative officer of the parole board "...to notify you that on October 30, 1972, the Adult Authority approved the Mutual Agreement Programming and Individual Voucher Referral as modified, in keeping with the Board's desires and the opinion from our Attorney General."

EN BANC ADULT AUTHORITY SESSION
ON MAP REVIEW -- JUNE 26, 1973

(Meeting Notes -- James O. Robison)

The board chairman opened with the suggestion we focus consideration on the three MAP cases then before the board, with discussion about their programs and general comments regarding MAP. The board would then retire to executive session to arrive at its position on the cases. He then read aloud letters of support endorsed by university faculty and the AFL-CIO. He remarked on a revision of the departmental administrative bulletin on MAP that had recently been prepared, and asked whether we (McDonald[1], Holt[2], Young[3], Robison[4]) wished to make a presentation or simply respond to questions from board members.

The question approach was chosen and the first question was: What are the selection criteria for MAP, and do the three cases submitted to the board meet the agreed upon criteria? McDonald read the criteria from his current draft, and a member then asked the definition of narcotics and wanted to know how firmly the board has emphasized narcotics as disqualifying during earlier discussions. McDonald replied that exclusion was defined in terms of addict or excessive use. The member pointed out that one of the three cases shows dangerous drugs and marijuana, but he decided it is not excessive.

Discussion shifted to the meaning of contracts and their legally binding qualities. We were told that the Attorney General advised them last week that the board could not legally make certain forms of commitment. The chairman desired a look at how the board's general commitment on the MAP agreement form was worded, but no copy was immediately at hand. A member wished to know what we're trying to prove by implementation of the program and McDonald explained. Member Hoover said that, in view of the Pruitt decision (a Morrissey-like judgment applicable to rights of prisoners on furlough), it would be necessary for the board to again see

[1]: MAP Project Coordinator for California
[2]: Department of Corrections researcher
[3]: Parole Division representative
[4]: MAP researcher

the general form for agreement they'd made about MAP, since he could not remember approving it. He suggested that, even if they had approved it, the subsequent Pruitt ruling required they reconsider MAP involvement and obtain the advice of legal counsel. Member Brown stated that they entered an agreement with us and that they should go ahead regardless of Pruitt. McDonald left to get a copy of the form, and member Kerr asked whether Holt had some input.

Holt said that the form for agreement leaves it completely up to board members as to what the specific terms are that they agree to sign to, and that the form is not inflexible. He mentioned that the other major issue concerns criteria for eligibility, and that the three cases before them are not "winners," but are representative of the types of case that will be available, and that if we're not willing to intervene with this type of case in an attempt to ward off a long repetitive future criminal career, but are instead to base decisions on the length of rap sheets, then there is really no point in going on with this. Hoover pointed out that two of the three cases before them were recommended for imprisonment by the Department, and that this was contrary to his understanding of who was eligible. Holt pointed out that the majority of first termers now being committed to prison are Z rejects,* and that the choice is between these and regular commitments who are likely to be third termers.

Hoover asked whether anyone had talked to Los Angeles police Chief Davis about the MAP program , and Young noted that three members of the Los Angeles P.D. are on the Community Correctional Center advisory committee, that they had discussed the project with their division commander, that Davis was known to support the Center but that it was unknown whether he had a position about MAP. Hoover wished to know what judges' positions might be, since MAP would be turning around cases recently sentenced by them, and he suggested their opinion be solicited. Young replied that they'd not been contacted, but could be, and that they were generally cordial toward and supportive of such programs. Someone pointed out the difficulty in contacting the large number of judges involved, and Kerr suggested it would be sufficient to obtain a sjpporting opinion from the presiding judge of the criminal division of the L.A. County Superior Court. Young agreed to obtain the endorsements of the police chief and presiding judge.

*Pre-diagnostic cases from the courts subsequently sentenced to a term of imprisonment.

McDonald returned with the agreement forms and noted that phraseology "legally binding contract" had been thrown out by the board months ago on the basis that the procedures were covered by the contingency parole date program already sanctioned by the board. Member Brown believed, from his reading of the agreement form, that the contract form entailed the prisoner's signing away rights to a Pruitt type hearing, and he believed that such waiver was not allowable. A member pointed to that portion of the form that states the Project Coordinator's decision is binding, and asked the intent of the phrase and whether it entails a non-legal delegation of authority. McDonald suggested the sentence could be rewritten for clarification. Hoover said it will remain necessary for the board to make the interpretations about whether a man has successfully completed the terms of his agreement. Members then argued about whether the man would be given a specified date at the time of agreement, or whether it would be possible to issue the date at the time of successful completion. The latter position was rejected. Kerr noted that the board might get into trouble if they took away a man's date when he'd made every honest effort but simply wasn't capable of completing the program. McDonald suggested that, for the limited number of cases in the project, it should be possible to set up the type of programs appropriate enough to ensure they could be completed by the person in question. Kerr continued to hold to the position that a man's lack of capability might not be discovered until after the commitment had been made.

Several members voiced the desire for an opinion from the Attorney General, but others suggested it might be possible to go agead and revise the offensive portions of the document. Hoover noted that prisoners formerly were allowed to waive rights, but could no longer do so, since they were looked upon as a "captive audience." Edmunds claimed that we'd earlier agreed to rewrite the last sentence because the authority was not delegable, and he noted that no such revision had been made. Member Brown suggested we ask the Attorney General what the board could sign, and member Kerr suggested that task not be left to us. McDonald commented that the form had been reviewed by a UCLA law school professor. A couple of board members stated that it would be necessary for the case to be brought back before them for final decision. Members then argued about whether the contract form, as worded, meant that the prisoner was signing away rights.

A representative from the AG's office was present and Holt suggested we might get together and reword it for board approval, but the chairman said there was not sufficient time then available. The possibility that a man in the Los

Angeles office of the AG could finalize acceptable wording was offered and rejected. It was suggested we strive at the meeting for agreement on the general idea, and later submit specific wording back to the board for final determination. The AG;s representative accepted responsibility for handling the task at a later time, or for referring it to someone else in the office to complete. Edmunds reiterated that either: "The Adult Authority at all times reserves the right to take final action with regard to any facet of the program," or words to that effect must appear in the final version. Members agreed that responsibility for ratifying the reworded version will be given to the chairman. Edmunds asked that the phase about "benefit to the State of California" be struck from the document. Kerr suggested rewording it to "may benefit" from "shall benefit." The matter was left unresolved.

Turning again to the cases being considered for MAP participation, Hoover remarked that addicts trained for nursing jobs would have trouble finding employment in that field. There was discussion about whether two of the three cases were to be looked upon as "narcotics" or as "pills," and whether the distinction mattered -- wouldn't a history of pills handicap someone in obtaining nursing work? Hoover next commented that the case expected to pursue plumbing in his MAP program had been around plumbing all his life, and it was puzzling why he needed it now or how it could make any difference. McDonald replied that the object was to get him licensed and into journeyman status, which would be a substantial career improvement, and he illustrated the man's potential for dramatic change by the fact that he'd just acquired a GED after a brief period of intensive study.

The chairman wished to know how long it would take to get the full sample of qualified MAP participants, since it had taken so long to obtain the first three. Holt replied that it has turned out to be more difficult than anyone had anticipated to find a sufficient number of qualified admissions, that some cases were being lost because of special problems in timing processing, but that with some reduction in sample size and an increased proportion of the sample taken from TFT's, we should be able to manage.

The chairman suggested that we get the matter of letters from Chief Davis and the presiding judge taken care of, plus the revision of wording to be provided from the AG's office, and he asked whether there were any further questions before going to executive session for review of the cases. Kerr asked how a case described as "marginal" for placement at CIM could be transferred to the Community Center. Holt noted that the guidance center judgment had been based on a history of escape as a juvenile. McDonald made the general comment that cumulative case summaries were prepared in haste and gave a particularly discouraging picture of the prisoner which was hardly indicative of future behavior, and that he placed more reliance on the judgments of institution staff made after they'd worked with and become acquainted with the case. Hoover disputed McDonald's claim that counselors may spend as little as five minutes in interviewing on admission, but Young, who had work experience in this role, supported McDonald and mentioned he'd processed a thousand cases in a three month period. He suggested that the guidance center counselors' prespective begins from the fact that the man is in prison, and that they are constrained to think in terms of alternative programs available in the prison setting. Member Hoover asked whether one of the cases was eligible for MAP in view of his having gotten a psychiatric referral, and there followed some difference of opinion about whether a subsequent psychiatric report had been obtained that lifted this cloud. Brown asked, if the board considered a MAP case and believed it necessary to set a date ten months away, would it be possible for us to take the man? Young pointed out that this would not be possible since it would require that the man remain at Chino for a period that would jeopardize our schedule.

Lynum summarized the general situation by saying that we've been seeking cases, and that the issue before the board is whether they can mentally go along with reprogramming such cases for the community, since they cannot expect to get much better ones in the future -- "The issue is simple -- will we go along or not?"

Following the meeting just reported, the California Parole Board did decide, once more, to "go along." The paragraph of standard MAP contracts dealing with interpretation provisions was rewritten, and one of the three cases reviewed at the meeting was subsequently awarded a contract. Even so, the difficulties in getting MAP programming underway in California were, by no means, yet over. Nearly two months after the crucial policy meeting of 6/26/73, the California Project Coordinator submitted the following memorandum "RE: Status of MAP Eligibles."

AMERICAN CORRECTIONAL ASSOCIATION

4321 Hartwick Road, Suite 212 College Park, Maryland 20740 (301) 277-3722/277-9028

PAROLE CORRECTIONS PROJECT

Leon Leiberg
Director

August 14, 1973

TO: Leon Leiberg
Project Director
College Park, Maryland

FROM: Don McDonald
California Coordinator
Los Angeles, California

RE: Status of MAP Eligibles

The following information is the present Status of the MAP Eligibles to this date.

X002
Subject was transferred by the Department of Corrections to a northern institution for disciplinary.

X003
Received orientation on 4/5/73. On 7/25/73 subject was granted a Parole Date of 12/21/73. On 8/8/73 subject was transferred to Central City Community Center and registered at Metropolitan Occupational Center in an Electrical Maintenance course. On 8/9/73 subject received a disciplinary action for use of marijuana and was returned to SRGC for action on the disciplinary. Subject appeared before the Disciplinary Committee and received a verbal reprimand and was then transferred to Program "D" pending action by the Adult Authority. Subject will appear before the Board on 10/19/73. This consensus at this time is that he has a chance of being returned to Central City Community Center with an extended parole date.

X010
Randomized on 4/16/73 and appeared before the Parole Board on 7/25/73. The Board members had different opinions on whether or not to parole him, after a lengthly discussion, he was denied the MAP Program. This subject was highly motivated and it was unfortunate that he was denied.

X011
Subject was randomized on 4/16/73 and was denied the MAP Program by the Adult Authority on 7/25/73. Subject was a marginal participant. His commitment offense was arson. He was transferred from Program "d" to SGRC, during his program development,

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AFFILIATES—Alabama Council on Crime and Delinquency ● American Association of Correctional Psychologists ● American Association of Correctional Training Personnel ● American Association of Wardens and Superintendents ● American Correctional Chaplains' Association ● American Correctional Food Service Association ● Association for Correctional Research and Statistics ● Association of Juvenile Compact Administrators ● Association of Paroling Authorities ● Association of State Correctional Administrators ● Central States Corrections Association ● Colorado Correctional Association ● Correctional Education Association ● Correctional Industries Association ● Correctional Service Federation-U.S.A. ● Federal Probation Officers Association ● Florida Council on Crime and Delinquency ● International Halfway House Association ● Maryland Probation, Parole and Corrections Association ● Middle Atlantic States Conference of Correction ● Missouri Correction Association ● National Association of State Juvenile Delinquency Program Administrators ● National Association of Training Schools and Juvenile Agencies ● National Correctional Recreation Association ● National Council on Crime and Delinquency ● National Jail Association ● National Juvenile Detention Association ● Parole and Probation Compact Administrators' Association ● Salvation Army ● Southern States Correctional Association ● Texas Corrections Association ● Volunteers of America ● Women's Correctional Association

for suspicion of arson at Program "D." Subject has numerous physiological problems.

X012
Randomized on 4/16/73 and on 7/25/73 received a Parole Date of 12/21/73. Subject transferred to Central City Community Center on 8/8/73, and was enrolled at Metropolitan Occupational Center. On 8/24/73, subject began classes at the Western Medical School for training as a Medical Assistant. It is anticipated that subject will start a part-time job at Cypress Hospital within a matter of days. Subject received assistants on this job offer from Central City Community Center Citizen Committee Advisory Board. To this date, subject is performing his obligations to the MAP Program in a commendable manner.

X013
Subject was randomized on 4/16/73. On 7/25/73, subject was denied the MAP Program.

X015
Subject was randomized on 4/16/73. On 7/25/73 subject received a Parole Date of 12/21/73. Subject was transferred to Central City Community Center on 8/8/73 and was enrolled at Metropolitan Occupational Center for a Graphic Arts course. During his initial testing his counselor and instructor at Central City Occupational Center stated that he is highly talented and could probably benefit more if placed on an OJT situation. The Graphic Arts instructor referred us to Mr. B.P. Steptoe, the owner of Executive Instant Printing. Mr. Steptoe was very impressed with X015's skill and motivation and agreed to start his training on 9/10/73. The OJT site has been approved by the staff of Central City Community Center and X015 is presently doing outstanding work.

X017
Subject was randomized on 4/9/73. Subject was inadvertently transferred to a northern facility in early May, 1973. To this day, we haven't been able to achieve his return to Southern California.

X018
Subject was randomized on 6/20/73. On 8/23/73 he was transferred to Program "D." Subject's Board Hearing date was on the 19th of September and was denied the MAP Program.

X019
Subject was randomized on 6/20/73. He was transferred to Program "D" on 8/8/73. He requested a continuous on his Parole Hearing on the basis of clarification on his holds from the State of Massachusetts. Subject was denied a waiver and was denied the

MAP Program. Mr. Hoover stated that he would not grant subject a Parole Date even if the holds were dropped.

X021
Subject should be transferred to Program "D" by 9/21/73.

X023
Subject was randomized on 6/20/73. He was transferred to Program "D" on 8/10/73. Due to a foul-up at the institution subject will be going to the Board next month.

X025
Subject was randomized on 8/21/73. On 8/3/73 subject was transferred to Program "D." His Board Hearing date was on the 19th of September and was denied the MAP Program.

X026
Subject was randomized on 8/21/73. His Board Hearing Date was on the 19th of September, the Adult Authority is pending further information on subject.

Respectfully,

Don McDonald
California MAP
Coordinator

Early versions of the California design called for randomized assignment and a study sample as high as 250, but the centerpiece, throughout, was the idea of an experimental sub-sample consisting of cases transferred directly to a community correctional center in Los Angeles within several months of their arrival, via court commitment or parole violation, in the prison system's Southern California Reception Center at Chino, and provided a training stipend to obtain preparation for a career of their choice under MAP. While under contract, they were still officially prisoners (education furloughees).

Although the Department of Corrections would provide community center bedspace and board at an estimated value of $90 per month, they required, in exchange, that Department of Labor funds be made available to pay for a "security package" -- salaries and overhead costs for correctional officers and a parole agent -- arrangements which caused delay but were consummated with less difficulty than those necessary for obtaining the funds for prisoner stipends. As the meeting notes presented earlier indicate, expending the prisoner training stipends proved more difficult than securing them, primarily because of the difficulty locating prisoners whom the parole board would accept into contract. In consequence, randomization was aborted and eligibility progressively re-defined in such a way that a number of cases finally granted contracts had already completed a considerable part of their expected prison stay with more than half the persons who finally obtained contracts having entered prison during 1972 or earlier. A total of 45 cases had proposed training programs fully developed for parole board consideration, but two were lost through transfer to other prisons before decision, and the board denied contracts to 18. Nine of the 25 who obtained a contract subsequently lost it, usually for excessive drinking, absence from the community center, or smoking marijuana. Nine of the 25 persons obtaining a MAP were members of minority ethnic groups and, among these, six lost their contracts. Twenty-one of the twenty-five cases granted contract had provided information at time of project intake on their highest earning level in the year preceding prison commitment. By claimed prior monthly earnings, we find:

PRIOR MONTHLY EARNINGS	OBTAINED CONTRACT	RETAINED CONTRACT	LOST CONTRACT
$701 or more	8	8	0
$401 - $700	8	5	3
$400 or less	5	1	4
No Information	4	2	2
TOTAL	25	16	9

Thus all eight with past earnings over $700 held their contract, and four out of five who had not reached $400 lost their contract.

The sixteen cases who completed contracts are listed below by type of occupation claimed prior to incarceration, and type of training (sometimes on-the-job) obtained through MAP.

CASE	PRIOR OCCUPATION	MAP TRAINING
A	Mobile home park developer	Mobile home salesman
B	Real estate manager	Diver
C	Supermarket manager	Air conditioning, heating
D	Auto wholesaler	Helicopter pilot
E	Computer operator	Computer programming
F	Heavy equipment oeprator	Automotive repair
G	Journeyman carpenter	Cabinetmaker
H	Salesman, truck driver	Office machine repair
I	Truck driver	Machinist
J	Cement finisher	Cosmetology
K	Paste-up and plate operator	Photo-mosaicer
L	Vinyl repairer	Machinist inspector
M	Janitor supply salesman	Sales representative
N	Cow milker	Helicoptor pilot
O	(No information)	Vending machine repair
P	(No information)	Taxidermy

The prison commitment offenses for these contract-completing cases included:

Manslaughter	1
Burglary	2
Auto theft	2
Grand theft-fraud	1
Forgery or checks	6
Receiving stolen property	1
Conspiracy to commit misdemeanor	1
No information	2

All subjects completing contract were sought for interview during July, 1974, subsequent to their completion of contract and release on parole. Three were unavailable -- one had moved to Florida (Case F), another was in county jail (Case D), and a third declared an absconder (Case G). Of the remaining 13, two were unemployed, two working part-time, and nine employed full-time.

One of the unemployed (Case C) had held a job as air conditioning mechanic for a month, at $6.25 per hour, but was then unable to work because of an asthma condition, and was hoping to be retrained in some other field; the second (Case B) had not worked since release, and was being supported by his family while he sought funds to resume his interrupted training in diving.*

One of the part-time employed (Case A) was working on commission in sales, recuperating from an eye operation, and uninformative about earnings since release. The other (Case H) was continuing his office machine training, loading freight at $7.10 an hour, and repairing typewriters on his own -- he had worked in three different typewriter repair shops since release, but left because of low pay ($3.50/hour).

Among the full-time employed, one (Case N) had first returned to the dairy, and was now working as a machinery mechanic in a cola factory at $4.80/hour, and hoping to obtain funds to resume helicoptor pilot training. Another (Case O) was self-employed in vending machine repair, but also working full-time as night manager in a restaurant at $150/week.

A third (Case J) had wanted training in brickmasonry but took cosmetology because it was available, and planned on resuming that training at a later time. Meanwhile, he had first taken employment working in construction for a relative at $8.00/hour, but was advised by his parole agent to leave that work because both (uncle and parolee) had drinking problems, so he was now working at $2.50/hour in a car wash and hoping to become assistant manager there.

A fourth (Case K) had been employed full-time since release in photo mosaic work at $2.45/hour. Another (Case L) had been working since release for $5.35/hour as quality control inspector in shipyard repair. Another (Case M) had held steady employment at $3.05/hour as a candy sales representative. Another (Case P) had decided, within a few weeks after release, to work on his own in a salvage business and to raise animals; he was earning $500-600/month.

One case (Case I) had changed jobs once since release, and was currently working full-time as a machinist at $6.25/hour while also holding a part-time position as cook, and meanwhile about to enroll at a junior college for further machinist training. The last (Case E) had held a position as instructor in computers at a technical school since release, was now earning $5.00/hour, and was enrolled at a local university to obtain a teaching credential.

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*It was later learned that training did resume, but was soon terminated by B's death during a diving exercise.

It is apparent that most of the California cases exposed to MAP programming who lost their contracts did so not because of problems in outside training, but because of misbehavior that the corrections agency decided couldn't be tolerated. Similarly, most of those who avoided such infractions or their discovery managed, upon completion of their contract, to establish themselves in training-related work. Still, it is important to note that the California MAP sample does not seem representative of prisoners in terms of background careers, and that not all the marketable skills they possessed after training can be attributed to their rather brief exposure to MAP. It is equally important to note that these training plans and arrangements, generally successful, were prepared rapidly by the state project coordinator, with minimal resort to consultation from specialists in vocational rehabilitation, or to formal assessment of vocational interests and skills.

GENERAL SUMMARY

WISCONSIN

MEASURES AT THE TIME OF ASSIGNMENT TO MAP

Based on the measures taken at the time of original assignment of prisoners to the comparison groups, randomization of subjects to experimental or control status appears to have been reasonably successful in establishing equivalent groups. While no statistically significant differences were found on any measure, the pattern of lesser differences suggests a slight initial advantage for experimentals that could be reflected in subsequent performance comparisons. Thus, they tended to be slightly older than controls, slightly less likely to have a record of prior incarceration, and slightly more likely to have been committed for a crime against person -- factors ordinarily associated with lesser risk of recidivism. In addition, they were slightly more likely to have completed high school and to have established a higher level of earnings prior to imprisonment, and slightly less likely to be members of minority ethnic groups -- factors one might expect to find associated with greater employment opportunity following release.

Within the experimental group, modest selection effects were operative to create differences between the sample who obtained and held contracts and those who either failed to enter or failed to complete a contract; only one such difference reached trend level significance -- a siphoning of prisoners with a prior record from the contract subgroup, which, in turn, inflated the difference between cases under contract and control cases to nearly twenty percentage points. Other screening effects, while less sizable, tend to invalidate any direct performance comparisons between the contract experimental subsample and the control sample.

MEASURES AT THE POINT OF PRISON RELEASE

MAP appears to have been unsuccessful in facilitating earlier prison release in Wisconsin. Despite noteworthy and statistically significant advantages of experimentals with contracts over experimentals without them, and a slight early lead in the release rate of experimentals over that of controls, later checks revealed essentially no net difference -- benefits to experimentals under contract were cancelled by losses to experimentals denied contracts or removed from them.

Among the experimentals who completed contract, only one believed he had stayed in prison longer because of that involvement; about one-fifth of the cases believed their contract had saved them no time in prison, one-fifth believed it had made a little difference, and two-fifths a lot of difference.

Controls and experimentals were equally optimistic about the value of their prison programs, with about three-fifths in each group believing they had been assisted at least a fair amount toward obtaining a decent job after release. Contract experimentals were more likely to hold this belief, but the effect was offset by reduced optimism on the part of experimentals without contract. Controls were slightly more likely to claim their prison work experience would be relevant to the job they expected after release, slightly less likely to claim prison coursework relevant to that job, and slightly more optimistic about how quickly they would attain a job and the level of earnings they would receive. While experimentals were generally and substantially more positive than controls in their beliefs about the benefits of MAP programming, experimentals and controls agree d that its greatest value appeared to lie in the more certain knowledge of a release date, the facilitation of plans dealing with matters outside prison, and the opportunity for earlier release. Both experimentals and controls were considerably less impressed with MAP's prospects for eliciting greater staff interest in helping prisoners, for providing better access to prison program opportunities, or for getting the programs to better serve the prisoners involved in them.

MEASURES FOLLOWING PRISON RELEASE

On measures of recidivism, very few subjects in either the experimental or control samples experienced serious difficulty, such as return to prison, during their first six months after release. Controls were slightly more successful than experimentals in remaining entirely arrest-free, holding a two percentage point lead after one month in the community, a six percentage point lead after three months, and an eleven percentage point lead after six months. None of these differences are, however, statistically significant.

Controls were also slightly more likely to secure a job within one week following release, and slightly more likely to be employed full time at the close of the one-month, three-month and six-month follow-up periods. Again, these differences are of small magnitude and not statistically significant. The full-time employment rate remained rather stable over all time periods at 62%-65% for controls and 53%-62% for experimentals, and employed experimentals were slightly more likely than employed controls to change jobs. A majority of both experimentals and controls claimed that prison training was unrelated to the jobs they had secured after release, with controls somewhat more likely to make that claim after one month in the community, experimentals more likely after three months, and there being no difference between experimentals and controls after six months. Few subjects -- less than 20% -- claimed any enrollment in any education or job training course subsequent to release, but controls were slightly more likely than experimentals to make such a claim during each of the follow-up periods. On earning level, experimentals established an early advantage, with 53% claiming wages of at lease $3.00/hour during some part of their first month in the community, compared to 39% of controls. This 14 percentage point gap closed to 10 percentage points by the end of the three-month follow-up period, and to 2 percentage points by the end of six months when about three-fifths of the members of both experimentals and controls claimed to have, at some point, reached the $3.00/hour level. None of these differences are statistically significant. Comparison of the distribution of occupational categories for post-release jobs with that of jobs held prior to incarceration revealed rather high stability. Experimentals increased their relative representation in the bench trades occupations, while controls did so in the machine trades and services occupations.

Within the experimental group, negligible differences existed, for the most part, between those who had completed a contract, and those who had not -- minimal differences were found at the end of six months on likelihood of arrest or likelihood of being employed; contract experimentals did, however, show a substantial advantage over non-contract experimentals on the measure of highest earning level achieved within six months in the community. A majority of contract experimentals (70%) also claimed, six months subsequent to their release, that MAP had helped them at least a small amount in making their post-release job experience either more pleasant or more productive. Surprisingly, one-third of controls were also willing to make that claim.

ARIZONA

MEASURES AT THE TIME OF ASSIGNMENT TO MAP

During the comparison of experimentals and controls on measures collected at intake, for the purpose of determining initial equivalency (or testing the adequacy of randomization), one sizable and statistically significant ($p < .025$) difference was revealed -- experimentals tended to have been committed to prison earlier and, thereby, to have served a longer period of their current term. Lesser differences, paralleling those found in Wisconsin -- experimentals slightly older, slightly less likely to have a record of prior incarceration, and slightly more likely to have been committed for a crime against person -- are in a direction suggesting a possible advantage to experimentals in terms of risk of recidivism. Again, as in Wisconsin, the experimental sample contained a slightly higher proportion of members who had completed high school, and somewhat fewer ethnic minority members than the control group. Pre-prison earnings, however, yielding a mixed picutre for expectations about future employment opportunity.

Within the experimental group, a statistically significant ($p < .025$) selection effect was found to be operative, on the commitment offense variable, serving to drain sex and drug offenders from the contract subgroup and to concentrate homicide, assault, and robbery offenders in that subgroup. This effect served, in turn, to introduce a statistically significant ($p < .05$) difference between contract experimentals and controls which makes it inappropriate to use these samples in direct performance comparisons against one another.

MEASURES AT THE POINT OF PRISON RELEASE

The MAP experimental group achieved a statistically significant and markedly faster rate of release from prison than controls and even experimentals without contracts had slightly more success than controls attaining release. This comparison, however, was based on time served between admission to the MAP study sample and release and fails to take into account the fact that experimentals had accumulated more time in prison than controls prior to MAP entry; comparison of total time served from prison admission to release indicated that experimentals spent, on the average, about three months longer in prison than controls. Given the flaw in the original randomization, it is not possible to claim any prison time reduction for MAP in Arizona. From the point of view of subjects who completed contract, three prisoners believed their involvement in MAP had kept them in prison longer, and one believed it had made no difference; about ten percent of the cases believed MAP had made a little difference getting them out earlier, another ten percent believed it had made a fair amount of difference, and over seventy percent thought MAP had made a lot of idfference in accelerating their release.

A substantially higher proportion of experimentals than controls expressed the belief that their prison programs would assist them at least a fair amount in obtaining a decent job after release; this difference reached trend significance level and was attributable entirely to that subgroup of experimentals who completed contracts. Experimentals were also more likely than controls to claim prison work experience and vocational training coursework relevant to the job they anticipated after release, even though experimentals who failed to enter or to complete contracts were less likely than controls to make that claim. Experimentals were also more optimistic about the level of earnings they would realize after release, but controls had higher expectations about the speed with which they would locate a job. In general, controls appeared to have as much faith as experimentals concerning the benefits of MAP program involvement and, as in Wisconsin, there was agreement that its value lay more in certainty of a release than in any improvement in prison program access or service or enhancement of staff interest.

MEASURES FOLLOWING PRISON RELEASE

On measures of recidivism, serious dispositions such as return to prison were infrequent within the first six months following release. With mere arrest (non-traffic) applied as an index of difficulty, controls were three percentage points ahead of experimentals in remaining arrest-free for one month, four percentage points behind at three months, and twelve percentage points behind for the six month exposure period. Experimentals without contract had a lower arrest rate, during each follow-up period, than experimentasl who completed contract; for the three month exposure period, this difference was nearly twenty percentage points and reached trend significance. None of the other recidivism comparisons between experimentals and controls, or between experimentals and controls, or between contract and non-contract experimentals, yielded statistically significant difference. On all follow-up measures in Arizona, findings suffer ambiguity because of incomplete availability of data.

Experimentals were somewhat more successful than controls in locating employment rapidly but, nevertheless, a higher proportion of controls than experimentals were holding full-time jobs by the end of their first month after release and this continued to be true at three months. By the end of six months in the community, experimentals had established a lead in full-time employment relative to controls; none of these differences is statistically significant. For the three follow-up periods, full-time employment rates ranged between 52%-72% for experimentals and between 56%-68% for controls; job changes by those who were employed were equally frequent among experimentals and controls. A majority of both experimentals and controls claimed prison training was unrelated to their post-release employment; contract experimentals were more likely than controls to claim training-related employment in the first month after release, but this difference vanished at three months and had reversed its direction by the end of six months. Few subjects -- about 10% -- claimed any enrollment in education or job training courses subsequent to their release; the differences between experimentals and controls were negligible in size and varied in direction over the several follow-up periods. On earnings measures, experimentals were initially behind controls, but improved their relative position in subsequent periods and established a final advantage. Among both experimentals and controls, the distribution of jobs by occupational category was fairly stable, with structural work occupations being most frequent both prior to and subsequent to imprisonment.

Within the experimental group, differences between the subgroup who had completed contracts and the subgroup which had not showed no reliable directionality across either type of measure or period of follow-up -- the contract subgroup were found, at times, to outperform the non-contract subgroup and, at others, to perform less well. Overall, the pattern of net differences between experimentals and controls suggests somewhat better performance by controls during brief community exposure, and somewhat better performance by experimentals after longer periods. No sizable differences were found, however, on any measure, and interpretation of findings is clouded by incomplete data supply on the one hand and evidence, on the other hand, that randomization had failed to produce equivalent comparison groups.

CALIFORNIA

It proved impossible, in California, to either adhere to a comparative experimental-control design or to involve a substantial number of prisoners in contract programming. Findings are, consequently, merely descriptive and for a small sample size.

Only twenty-five contracts were obtained and nine of these were cancelled because the subjects were discovered in rule infractions at the community correctional center. Cancellation fell, most frequently, on minority group members and lowest (as measured by prior earnings) socioeconomic status. The sample of sixteen who completed contracts was atypical of incarcerated offenders, containing a disproportionate number from white collar backgrounds and committed for forgery.

Follow-up on fifteen of the sixteen who completed contract shortly after such completion found nine working full-time, two part-time, two unemployed, and two in parole violator status. Excluding the violators, nine of the remaining thirteen had managed to secure training-related employment for at least a brief period, three were involved in continuation of training, and four more were hoping to resume training. While some of the subjects were trying to advance themselves in an occupational field related to their past work history, several appeared to have successfully embarked on an entirely new career.

CONCLUSIONS AND RECOMMENDATIONS

A first reaction to the absence of dramatic evidence of positive program effects in the area of post-release employment adjustment and recidivism might be the judgment that the MAP approach is worthless and ought to be abandoned. I think that reactionwould be a mistake, and will attempt to explain why.

The potential number of completed contracts was 150 in Wisconsin, 130 in Arizona, and, by a different form of reckoning (i.e., the number of cases for whom the project coordinator initiated program development effort) 45 in California. The actual number of successfully completed contracts was substantially short of those potentials, being 68 in Wisconsin (45% of those possible), 75 in Arizona (58% of those possible), and 16 in California (36% of those possible). A major consequence of these high levels of attrition was that, in the basic comparison of post-release performance differences between the full experimental and control samples, there was no opportunity for many experimental subjects to contribute directly to any program effect that might exist, and a heavier burden fell on those actually engaged in contracts to contribute to the performance measured on the total experimental group. In Wisconsin, 24% of cases either declined initial involvement, or voluntarily removed themselves from contract consideration at some point prior to formal signing. A parallel loss occurred in Arizona (23%), despite the earlier winnowing of those who did not complete the intake questionnaire. In the presence of a continued display of interest on the part of the prisoner, an additional 18% of cases in Wisconsin, and 15% in Arizona, were denied contract entry by the parole boards in those states, and only 25 cases in California progressed to the point of a signed contract. The major single factor involved in prisoners removing themselves from consideration, or being removed from consideration by the parole board, was time. Prisoners who believed they would soon be released regardless had no incentive for involvement, and prisoners who the board deemed had served an insufficient period of their terms were unlinely to be accepted.

Attrition also took place after formal contract entry, with removal from contract programming ordinarily being cancellation by the authorities rather than voluntary dropping out on the part of the prisoner, and cancellation nearly always resulting from a disciplinary infraction rather than failure to satisfy some treatment or training term of the contract. There was considerable variability among the states, with contracts completed by 94% of those who entered one in Arizona, 78% of entries in Wisconsin, and 64% of entries in California.

The duration of individual contracts ranged from one to nine months, with a majority being fewer than five months in length. If we were to suppose that beneficial effects were taking place, rather modest expectations would nevertheless be in order because of the limited time available for them to be imparted.*

Apart from involvement and assistance provided by the state employment service in Wisconsin, (a service which was offered to both experimental and control subjects) plus some effort to increase counseling sessions for contract cases there was no augmentation of resources for contract prisoners in Wisconsin or Arizona -- to a large extent, contract terms in both those states consisted of an agreement to continue existing program activities, rather than to undertake new ones.

It should be kept in mind, however, that the essence of MAP programming concerns not so much the availability of enhanced program resources as it does the nature of the formal relationship between the prisoner and whatever resources happen to be currently available.

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*The "drop-out" rates from contract programming, as well as the brief duration of contracts, must be interpreted within the context of special project conditions which decreed a deadline for contract completion so that follow-up data could be acquired within the grant period, and thereby necessitated that contract entry take place relatively late during prisoners' terms of imprisonment. An experiment more closely approximating routine conditions has been conducted in Michigan, and the Michigan findings show, as one might expect, lower rates of attrition in the contract entry phase: Of a sample of 247 cases committed to prison during early 1973 and assigned to contract experimental status, only 7% declined entry and 11% were denied entry -- rates of initial loss about half as large as those in our Wisconsin and Arizona study samples. In a check made one year after inception of the project, or nine months after the signing of the last of the 202 contracts, it was determined that 20% had subsequently been cancelled, with over 90% of cancellations arising from disciplinary infractions. Removals for failure to meet other contractual obligations were, as in our own study sample, quite rare.

From the points enumerated above -- high experimental attrition, brief contract duration, unenhanced program resources -- plus several others that could be listed -- short follow-up period, data loss, crude criterion measures, overly simplistic analysis, suspect randomization in one state -- one might construct a defense of the program, arguing that effects were produced which our research techniques failed to register, or a defense of the concept, aruing that the constraints were too great to yield an effect -- that the program concept was not fully implemented. If I were to attempt to build a defense, and I shall, I would begin, instead, with the argument that enhanced employment or reduced recidivism are the wrong objectives to set for mutual agreement programming, and that far too much importance is given to claims about augmenting or allocating program resources in prison on the justification or promise that they will better accomplish those objectives.

How then, would I go about justifying the expenditure of the more than one-half million dollars invested in the Parole-Corrections Project?

I would like to emphasize the fact that, at the time this demonstration was initiated, there were two fundamental unanswered questions, rather than just one. There was not only the question of whether mutual agreement arrangements for programming would lead to enhanced performance, but whether the rigors and implications of a signed and binding contract could be tolerated at all in a system accustomed to maximal discretion and unilateral control. The project has managed to demonstrate that this transition can be made, and that it can occur without producing major disruption. The importance of that finding should not be discounted, since it has established a foundation that many seriously doubted could be built -- a foundation that, by virtue of the increased certainty it provides both prisoners and administrators, enables both to make more realistic plans on the basis of more informed forecasts. I think it not unreasonable to assume that this fact may, in itself, account in large measure for the favorable attitudes expressed toward contract programming by, not only the recipients, but other prisoners, as well. I think also that we should not underestimate the potential (as yet unexploited) that the model offers those whose duties require the planning, the coordination, and particularly the scheduling and workload calculations for prisoner programs. That potential lies not only in the opportunity to make better estimations and projections, but in the forceful reminder that the model provides of the need for such estimates and projections. It is time that the taxpayer received a better accounting of what actual services his tax dollar for "correctional programming" purchases, and time prisoners received a better accounting of the reasons why decisions about their length of imprisonment should be influenced by whether they invest their energies in partaking of those services, and why they are not provided earlier and clearer answers about how long they will be kept in prison if they do not invest themselves.

From these sorts of consideration, and the definition of objectives that follow from them, rather than from those which accept rehabilitation as the primary aim, the Parole-Corrections Project may be credited for having made some substantial contributions to clarification of issues, and having accomplished a great deal in proving that the reasons usually offered for denying prisoners advance knowledge of their dates of release are suspect in their validity, unless that reason is "to make things more unpleasant for them."

Without question, the most important project finding, to date, is that correctional administrators and parole board members have demonstrated that it was possible to set in advance and adhere to commitments made to release prisoners by particular agreed-upon dates in the absence of major disciplinary infractions. This finding is particularly relevant to a major concern of the U.S. Department of Labor -- that it be possible to schedule investment of funds in prisoner training under reasonable expectation that prison release might coincide with completion of training. Second, it has been demonstrated that prisoners accepted for contract programming were quite unlikely to voluntarily drop out of this arrangement, though it must be acknowledged that they may have had less than full confidence that they would be allowed to "revert without prejudice to the general population."

Further, it is evident that, given the selection procedures employed, a substantial proportion of prisoners who entered contractual agreements -- 80%-90% -- were able to fulfill on schedule, and to the satisfaction of institution and paroling authorities, the terms of their agreement. Next, it should be pointed out that the selection procedures employed did not involve stringent screening -- apart from statutory exclusion factors, basic eligibility for project inclusion was extended to a wide spectrum of incarcerated offenders. While a moderate proportion of eligible experimentals -- about one in four -- was found not to be sufficiently interested to pursue contract negotiations to completion, it was also found that three out of every four who arrived at the stage of final commitment were found acceptable by institution and parole authorities. These findings, taken together, appear to document the feasibility of the contract model and to suggest it has potential for widespread applicability for increasing certainty of release date. This is not to say that it is either the only or the best available alternative to current prison programming and parole granting practice, but that it is an alternative which is definitely worthy of consideration. The most obvious drawback to the model, as now in operation, is its vulnerability to coercive and to discriminatory applications; further safeguards, such as more adequate arrangements for appeal, should be considered for incorporation in the model.*

*In the demonstration project phase, the contract paragraph containing interpretation provisions was worded differently in each of the three states, with the Wisconsin version most stringently stated. Compare the following statements concerning resolution of disputes and the authority of the Project Coordinator:

WISCONSIN

"All questions, issues or disputes respecting determination of successful completion of any *contract program* or service objective shall be decided by the *MAP Project Coordinator*...The decision of the Project Coordinator shall be in writing and shall set forth the facts on which it is based and shall state the reasons for the decision. The Project Coordinator's decision shall be final and binding on *all* parties hereto."

ARIZONA

"All questions, issues or disputes respecting determination of successful completion of any *program* or service objective shall be decided by the Board of Pardons and Paroles...The decision of the Project Coordinator shall set forth the facts on which it is based and shall state the reasons for the decision. The Project Coordinator's decision shall be final and binding on all parties hereto, *except* the Board of Pardons and Paroles."

CALIFORNIA

"All questions, issues or disputes respecting determination of successful completion of this *Agreement* by the Participants shall be submitted to the MAP Project Coordinator for his *recommendation* to the Adult Authority...The recommendation of the Project Coordinator shall be in writing and shall set forth the facts on which it is based and shall state the reasons for the *recommendation*." (Our underlining.)

There is a manifest lack of reciprocity in the Arizona and California versions, where one party to the bargain also serves as arbiter. Since Project Coordinators are likely to be drawn from and to have some identification with the correctional system, even the Wisconsin model is in this respect, less than satisfactory. This fact was recognized in another (unexercised) provision of Wisconsin MAP documents:

ARBITRATION

"An arbitration board will be established from among corrections and parole and other collateral agencies, eg. State Board of Education, State Employment Service, State Vocational Rehabilitation Services, State Planning Agency, etc...The board will set up arbitration steps and procedures and act as a court of last resort in arbitrating contract renegotiation which has come to an impasse."

I would argue, then, that this is a base worth building from, rather than one due for abandonment, and I would advocate not replication of the obvious, but further development of the possible. I would see these developments taking several fairly specific directions, generally subsumed under the heading of a further shoring up on the accountability factor through:

1. Linkage of the contract model to an adequate machinery for redressing prisoner grievances, including access to outside arbitration for the resolution of disputes. Until that step is taken, we are in danger of confusing ritual with substance, and remaining in a situation of very lopsided bargaining power -- a situation unsuitable to a true contract model.

2. Reassessment of whether the activities now undertaken under the auspices of "correctional programming" are to be justified (and can be justified) in terms of their rehabilitative promise, or are to be accounted for, instead, as a means of:

   a. meeting institutional convenience, or
   b. relieving the monotony of incarceration, or
   c. satisfying some diffuse cultural prerogative concerning what prisoners "ought" to do while confined.

3. Enlistment of more of the community services and resources deemed suitable for generally serving persons in need, on the grounds that offender status, in itself, is not a particularly relevant distinction for human service delivery systems.

4. Extension of the contractual concept of bargaining power beyond the level of individual prisoner confronting parole board and prison administration, to include consideration of the reasonable possibility of arrangments for collective representation.

Finally, in behalf of the model as already implemented, I would assert that the demonstration project has, accomplished its primary purpose -- that of improving communication and coordination among those responsible, on the one hand, for administration of prison programs and those responsible, on the other hand, for release decisions. Perhaps, under the constraints within which this initial phase was to operate, that is, in itself, quite enough of an accomplishment.

AFTERWORD

When first invited, in July, 1972, to be considered for the position as research director for the Parole-Corrections Project, I flew to College Park for three days of document familiarization and conversation with the project director. Being unable to sleep on the second night, I sat down to draft my views about the then-existing research design and to suggest various possibilities for coping with problems of data collection and analysis, and for further specifying a statement of the Problem. By page 9 of the draft, I had gotten myself boggled:

Fred Cohen, in his *The Legal Challenge of Corrections*, defines a contract as:

> "...a freely bargained-for, mutually acceptable, agreement supported by a valuable consideration and arrived at by parties who possess some equivalency in bargaining power."

I am having some difficulty in applying the phrase "valuable consideration" to the model described in the project proposal. One of the conditions of imprisonment is the duration of incarceration, and reduction of that duration is obviously a valuable consideration.

It is impossible, however, to assess the actual value of this particular type of "valuable consideration" without some knowledge of the amount of reduction in duration that is involved. But this particular ingredient seems entirely lacking from the model, which yields only a "legally binding"* specification of the time at which one *will* be released if certain conditions are met, without an equally definite specification of when one *would* have been released if the formal obligations were not incurred. One can only speculate about, rather than determine, the magnitude of difference for a given individual, though our operational model will permit us to determine aggregate worth by comparison of time served by the experimental and control samples. That we *could* do so does not necessarily mean that we *ought* to do so; the suggestion occurs in some of the state models, but not in others that such comparison is to take place. About the only value I can see is that, once made, inmates could be made aware of the average value of contracts undertaken in the past, in terms of time savings, and be in a slightly more informed position to consider whether they wished a contract. They would still not, however, have knowledge in these terms of the value of their own particular contract. I recognize that the concept of prescription programming includes the offer of additiona, and more immediately tangible valuable considerations or rewards, such as special privileges or more pleasant conditions of confinement, and that the inmate may take on obligations beyond expanding his energies in vocational training (e.g., he may endure some amount of some "treatment") in exchange for the total package of valuable considerations. By that point in the model, however, we have ventured rather far from the Problem of getting vocational training completion to coincide with the date of release.

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*A great deal more legal research is necessary in the affected states to determine the means of establishing "legally binding" contracts, and the position that courts are likely to take with regard to their strength.

The concept of contract or MAP is, for that matter, not the only feasible way to solve the Problem. The Problem arises, just as does the difficulty involved in assessing value of contract, from the absence of knowledge of date of release without training or without contract. Just as date of release might be adjusted to completion of training, so might completion of training be adjusted to date of release. With a knowledge of the latter, one could, on the basis of an estimate of training duration, make fairly intelligent decisions about when training should be initiated if it is to coincide with release. This approach omits, of course, the reward of reduction in time served as the exchange for successfully completing training,but I have some difficulty in understanding why it is necessary or appropriate to offer such a reward, since the opportunity to secure a skill is generally looked upon as being, in itself, a valuable consideration, and that value is, in fact, tainted, by the offer of reduction in time served. One might, however, be induced to undertake valueless training if that could reduce the period of confinement, and one might accept it as a good bargain even if it could have no bearing on increased opportunity for employment. ("Why did you take carpet-laying training since there is little chance to apply it?" "So I could get out earlier, you damn fool. I would have taken flagpole sitting if they had suggested it.") Perhaps I exaggerate the coercive implications of the contract model. I realize, of course, that the inmate is "free" to reject the bargain, or to attempt to negotiate a better one, but to return for a moment to Cohen's definition, I am not yet sufficiently convinced about the inmates "equivalency in bargaining power" -- it is an enormous leap from a condition of minimal power to a condition of equal power, and the dialogue recorded in pages 66-76 of the Proceedings (Resource Document #2) continues to aggravate my suspiciousness. My attitude toward behavior modification has been made rather well known and I smell a heavy whiff of that in prescription programming notions. Bargaining power ordinarily arises out of a banding together of persons with closely similar interests or grievances, and my own guess is that the attainment of equivalency will not be readily a-tained merely through implementations of the project model. Until the "Guidelines"* achieve substance, I am pessimistic and doubt that we shall secure much more than a new form of ritual for regulating deprivation of liberty. I would like much more than that (and will remain on guard to make it more than that) even though I can appreciate that the open declaration of a date for being freed is a substantial benefit to inmates.

Since the project appears to be quite definitely committed to a MAP model, I would prefer to invest the entirety of research resources in documenting, describing, and developing a theoretical framework about the practices and meanings of entering contracts and satisfying their conditions in the prison setting. I would dig deeply into that and nothing other than that. I would abandon the experimental-control model and advise that it not be implemented until we possess a much better understanding than we have today of the rudiments of what we are talking about. A test of efficacy seems definitely premature. Even a test of feasibility is probably premature, though it is probably our best opportunity for exploring this realm. We must argue strongly and apprise those to whom we are accountable that too much is being assumed and that, on our present course, too little will be determined.

Nearly three years have passed since that sleepless night. In dreams on other nights since then I have been summoned from a cell and invited to participate in drafting some constructive program of activities for myself to be linked, in exchange, with a going-home date. Each time I find myself able to utter only three words ("No, thank you") and each time, while returning to the cell, voices in the background are heard saying..."arrogant, yes, but lacking in dignity."

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*DOL/DHEW "Suggested Prison Inmate Training Guidelines," reprinted as Appendix C, Parole-Corrections Project Resource Document #2.

APPENDIX I

SAMPLE CASE NARRATIVE
FROM RESEARCH DATA SYSTEM

RESEARCH MEMORANDUM

James Robison
April 28, 1973

SAMPLE MAP CASE

(The following case has been constructed from information available from the following MAP documents: Status Report Sheet, Intake Questionnaire, Contract Term Sheets, Contract Problem Resolution Form, Intensive Employment Placement Release Form, Prison Release Questionnaire, and first Follow-Up Form.)

The subject, whom we shall call Adam Able, was assigned to the Wisconsin study sample last September. He was twenty-six years old, black, and had spent the past year in prison on a conviction for statutory rape. It was Adam's second term in prison, and he had never been paroled on the current sentence nor told by the parole board when he might expect release. His minimum eligible parole date was one month away, and his own expectation was that he would be released in five months if not issued a MAP contract, and that he would serve the same period even if he secured a contract. Though he expected to serve somewhat more time than other prisoners with a similar record, and indicated willingness to go to a fair amount of trouble in exchange for knowing when release would occur, he thought it would be worth very little effort to reduce his prison stay by one month, and only worth "some" effort to reduce it by three months.

While Adam did not believe that a MAP contract would significantly alter either his period of imprisonment or his program of prison activities, he nevertheless was favorably inclined toward the project, stating: "I think it can be a beautiful program if it works, I feel that they should of had something like this before now." He expressed confidence that the parole board and institution would honor contractual agreements.

Adam had completed high school and, before coming to prison, had last worked as a machine operator. He had held that job six months, working full-time and earning $3.75 an hour. Before selection for MAP, he had been studying electronics -- TV and radio repair, and, while he did not at that time have a release job offer, he expected his first post-release job to be in the area of electronics, and he also planned to obtain further education and training in that field.

Mr. Able was selected, by randomization, into the MAP experimental group, and sought a contract which would continue his full-time enrollment in the institution academic (typing) and vocational training classes (electronics, and masonry). After discussion with the MAP project coordinator, continued attendance at Alcoholics Anonymous was added as a contract term, and the contract as subsequently developed and signed in later October, came to also include weekly counseling sessions, a more explicit statement of the areas of training proficiency to be established, and an understanding that Adam would be enrolled in a technical college after release. A release date of January 17, 1973 was stipulated in the contract.

The contract period passes without incident and Adam was released on schedule to begin classes at the technical college. Because of this involvement, no utilization was made of the Intensive Employment Placement service. At the time of his release, Mr. Able noted that his electronics training would continue, and that he expected to secure part-time employment as an electronics helper within one to two months after release, at an hourly wage between three and four dollars, and monthly take home pay between two and three hundred dollars. Prior to his admission to prison, he had received no training or work experience in that field.

With regard to his contract involvement, Adam credited MAP with making a lot of difference by making him more certain of his release date, by getting him more interested and working harder, and by helping him plan and make arrangements outside because he knew when he'd be leaving. He thought it had made a little difference in getting prison staff more interested in helping him, and in accelerating his release from prison by about one month. He saw MAP as making no difference in getting him into programs he couldn't otherwise have gotten, or in making the programs he was in work any better. He gave MAP no credit for making his time in prison pass easier, for improving his chances of getting a good job on the outside, or for helping him stay out of prison in the future. Asked what he thought was good about MAP, he replied: "Some people get out earlier, and for others nothing." He said he saw nothing bad about MAP and had no ideas for its improvement.

Commenting on his total prison experience and programming, Adam indicated that it had made no difference in helping him to get a decent job. He believed low income or lack of work had no causative connection with the offense that brought him to prison, and he anticipated no difficulty in avoiding criminal behavior in the future.

Mr. Able completed his first month on parole without arrest or technical violation of parole. He was still enrolled in electronics training at the technical college, and had, three or four weeks after release, obtained a part-time job as a repairman; he was working less than 20 hours per week for between $2.00 and $2.50 per hour. Asked once more about MAP, he credited the program: "gave me something to work towards," and expressed a belief that it had helped quite a lot in making his job experience since release more pleasant or productive.

APPENDIX II

WISCONSIN INTENSIVE EMPLOYMENT PLACEMENT

WISCONSIN IEP (INTENSIVE EMPLOYMENT PLACEMENT)

The Wisconsin MAP model includes arrangements for Intensive Employment Placement (IEP) -- services concentrated in the 60 day period preceding release to parole, and offered to both Control and Experimental subjects of the MAP study sample. Services are provided by the MAP project staff and the Wisconsin State Employment Service; the first 30 days are focused on assessment of skills and familiarization with employment application and interview procedures, and the second 30 days are devoted to arrangements for actual job interviews.

As stated in a MAP-IEP bulletin of May 14, 1973, "There are several goals in IEP. The primary one is helping men still in the institution phase of their sentence obtain offers of employment to which they can go upon their release to parole. Another goal is to obtain job interviews in the community during the 30 days prior to parole."

IEP Release Forms were received for 128 of the 129 subjects released through September, 1973. Inquiry on the forms is addressed to seven successive thresholds of program accomplishment:

1. Was the prisoner offered IEP?
2. If offered, did he accept involvement?
3. If accepted, were IEP services actually provided? (vocational aptitude testing, counseling or training in job finding or job keeping, and/or efforts to assist in arranging job interviews)
4. If any services provided, were any interviews actually conducted?
5. If conducted, did interview produce a job offer?
6. If job offered, was prisoner slated to begin employment?
7. If slated, was the job related to prison training or experience?

Except for the transition between stages 3 and 4, above, a majority of the cases arriving at any particular stage suceeded in traversing that stage. The major problem in the system was found to be securing job interviews for prisoners -- only one-third (31%) of the cases for whom services were provided obtained a job interview prior to release. Even if it were possible to overcome this particular problem, the progressive winnowing at other stages still produces a high level of attrition.

It was possible to offer the services to 91% of the subjects sought for IEP. Seventy-five percent of those to whom an offer was made accepted. Some form of IEP service was provided for 91% of those who accepted IEP. Of those subjects for whom it was possible to arrange a job interview, 76% received an offer of employment. Eighty-four percent of those who received such offers were actually slated to begin employment, and two-thirds (68%) of those so slated to begin had obtained work in an area related to prison training or experience. Nevertheless, only eleven of the 128 cases, or 8-1/2% of the total, arrived at the point of securing a training-related job on the basis of an IEP arranged job interview.

ACA-DOL FORM # 61 (1/29/73)

INTENSIVE EMPLOYMENT PLACEMENT RELEASE FORM

NAME_______________________________________ PROJECT ID#_________

SERIAL #____________________ DATE FORM COMPLETED________________

[Continue to answer the following questions as long as the answer is "YES". If you answer "NO" to any question (except a or b in Q 3), explain at bottom if required, and DISREGARD SUBSEQUENT QUESTIONS.]

1. Was the prisoner ever offered IEP?
   _____YES _____NO [Explain and Stop]

2. Did the prisoner accept the offer of IEP?
   _____YES _____NO [Explain and Stop]

3. Which of the following services were actually undertaken for this prisoner?
   a. Vocational Aptitude Testing ___YES ___NO [Go On]
   b. Counseling or training in job-finding or job-keeping ___YES ___NO [Go On]
   c. Efforts to assist in arranging job interviews ___YES ___NO [STOP]

4. As a consequence of IEP effort, were any job interviews actually conducted between the prisoner and potential employers?
   _____YES _____NO [Stop]

5. Did any job interview secured by IEP result in an actual offer of employment?
   _____YES _____NO [Stop]

6. Is the prisoner now slated to actually begin employment as the result of an IEP arranged job interview?
   _____YES _____NO [Explain and Stop]

7. Is the IEP arranged job that the prisoner will be taking in any way related to training or experience received during his present stay in prison?
   _____YES _____NO [Stop]

   a. Type of job taken______________________________________

   b. Relevant prison training or experience___________________

Explanation of "NO" answer if required__________________________

APPENDIX III

ONE PRISON TRAINING SUPERVISOR'S VIEWS ON MAP

It seems to me that most of those selected for the program have turned out to be more concerned about how soon they could be released, rather than being really, deeply and sincerely concerned and involved and motivated in diligently pursuing the goals or terms of the contract they had agreed upon.

They tell me that they felt pressured into signing up for various programs, such as group counseling, some school programs, etc., and would not have signed up if not for this pressure. As a result, they are, for example, not motivated for individual or group therapy and although they attend the meetings or individual sessions, there is a much more pronounced resistance, a feeling of being required to be there and resenting it which greatly hampers the therapy effort. Perhaps this would not be true if the program just existed in the institution and was there for any inmate to truly think about his needs and input, come forward voluntarily and "pick up the program and run with it." Then he would truly have seen his needs and truly volunteered for those programs which he saw as being beneficial for him and would be motivated from the start. Perhaps it is just accident that had so many unmotivated people in individual and group counseling, or perhaps their abilities were limited, but I definitely got the impression that signing up for what I believe in many instances was an earlier release date than what normally would have been the case (because of the 8-31-73 deadline) was the main motivator, not the benefits of the program which could be reaped for oneself. In other words it was difficult "to pay the piper." Due to the lack of motivation they were harder to work with.

I have seen cases who had been adjustment problems who, after the contract was signed, became somewhat less problems in term of acting out and receiving conduct reports but there was no substantial change in attitude. A kind of begrudging exertion of some control over oneself. And I am not at all convinced that "breaks" were not given more often to MAP clients than to the population at large, i.e. is in terms of conduct reports not being written, staff going to extra lengths to help an inmate to avoid violating his contract. In some cases I am certain this is true.

While the MAP program, especially when coupled with the IEP program, may prove to be more beneficial than without it (which I doubt since most everyone at W.C.I. gets the program he wants anyway and I believe the Parole Board recognizes progress), there is little doubt that the program has been disruptive to some of the institution normal procedures. This is especially true in scheduling and rescheduling contractees for various required procedures such as parole planning, programs changed because of renegotiation and the changing of dates, etc. However, I do not believe this would be true if there were adequate numbers of staff to handle both programs. What we tried to do in effect was continue present parole and program procedures while at the same time take on a differently conducted way of proceeding, all at the same time. There is not a sufficient number of staff for this, especially with so many other additional programs and precedures being initiated at the same time. They may all prove to be excellent, all I am saying is that there is just a limit on how much a given number of people are able to do. Because of this, somewhat contrary to what I stated above, I believe there have been some instances where non-MAP clients have been short changed, and I get a definite feeling that other inmates must feel somewhat "left out." I say this because non-MAP contractees are seeking more attention than ever before and this seemed to coincide with the initiation of the MAP program. This may, however, be just coincidence and due to other factors such as increased awareness, increased aggressiveness, etc., may be due just to the changes of the times, but I think it could be related to a feeling of not wanting to be overlooked, forgotten, left out, by those who don't have a contract. This created tremendous pressure on existing staff who already felt in many instances they were working under duress. Nevertheless, I can't think of an instance where there was a lack of cooperation, in fact, the staff seemed to want to give it a good try.

I think if the program were truly voluntary and just there in the institution for those who wanted it, it would work, but picking names out of a hat, calling them in and pushing through a program for a man, if he is not motivated, I feel, except for institution discipline, etc., is somewhat useless. I have not found this type of MAP client to be more enthusiastic about program involvement. (By the way, I do not like your questionnaire as I cannot convey fully what I wish.)

MAP cases do not necessarily show more respect for staff but they do not necessarily show less either. I don't think it makes a difference, except where there is already a very poor attitude and the inmate is forced to exhibit at least a modicum of respect, or, the absence of action out.

I think it is definitely false that only the better prisoners got contracts in this instance. They might have if the program was totally voluntary. Just because the motivated would tend to take part while those with poor attitudes would not.

I really can't say what influence MAP men have on the others. Others are not eligible right now anyway. I think if everyone felt he could get as good a deal as most of the originals got, there would be quite an influence. There may be an influence if the Parole Board treats non-contractors the sam way it treats contractors. I think they do to a degree by recognizing and rewarding progress but I think the 8-31-73 deadline caused many to get a much better deal (earlier release) than they otherwise would have.

One thing I like about the MAP program is that it relieves me of duties such as preparing parole summaries, thereby, freeing up time for more individual involvement with clients. On the other hand, more time is taken up with keeping constant track on progress, referrals, etc.

I don't think MAP cases make my efforts more productive in those instances where there is little or no motivation and the client is in my office only because he feels he has to be. In this instance casework on an as needed basis is superior.

I think most all inmates have the availability of the same programs, MAP or not. At least this was true. To what degree others are being excluded from programs because they are filled with MAP clients, I cannot say. I believe it is true in some instances, probably too many.

MAP cases feel under more pressure than others to e.g. avoid conduct reports, so they do harder time, but I don't think they are disappointed because they are happy with what are probably recognized as earlier dates than what they otherwise would have had. I think that, due to the lack of sufficient numbers of staff, the true quality programs that could be available if sufficient numbers of staff were available, are not really existant. Therefore, staff is spread thin and I think in this respect, more was promised (or expected) than can possibly be delivered with present numbers of staff.

MAP cases do not necessarily obtain more insight, this is up to the individual.

I think the answer to this thing is to simply make the program available for those who want it; those who are motivated will come forward and see the advantages for themselves, will get involved in it, even if they have to give a little more than they might have. Then, since it is a contract, they will know in advance when they are going home and the institution will be bound to see that the client gets what has been agreed to, but there is going to have to be sufficient staff to do the job.

I hope that all inmates will eventually get IEP. Obviously, a man could obtain a college degree in prison but if not given a job opportunity, all is for naught.

APPENDIX IV

MAP EXCERPT FROM

ARIZONA PAROLE BOARD ANNUAL REPORT

EXCERPT FROM: STATE OF ARIZONA, BOARD OF PARDONS AND PAROLES' ANNUAL REPORT, JULY 1, 1972 - JUNE 30, 1973, PAGES 5 AND 6.

Pertaining to parole, an experimental release program was introduced to the Arizona correctional process under identity of Mutual Agreement Programming. This concept was aired in its entirety at a National Workshop for Corrections and Parole Administrators held in New Orleans in February 1972. The correctional administrators and Board Members present discussed their respective problems in cooperative communications and chose Mutual Agreement Programming as an appropriate vehicle by which to formulate acceptable guidelines for opening communicative channels.

Arizona, through the Board of Pardons and Paroles and the Department of Corrections, was one of three states to enter into this research project with the American Correctional Association. This project commitment is scheduled to terminate in February 1974.

The Arizona Board of Pardons and Paroles originally accepted the plan provided none of the inmates chosen (by drawing their names from out of a hat) were in prison on any charge of violence, sex offenses or drugs. An agreement was made under these conditions between the Arizona Department of Corrections, the Board of Pardons and Paroles, and the American Correctional Association who was financing this experiment through funds appropriated by Congress.

The American Correctional Association objected to the conditions imposed by the Arizona Board of Pardons and Paroles, insisting that this was wholly an experiment to determine the feasibility of releasing all elements of prison inmates under such an experiment and insisted that we go along, since it was an entirely new experiment; that we were one of the first states to enter into the experiment; and to enforce such conditions would not provide for an overall study of the entire population of the prison. The Arizona Board of Pardons and Paroles very reluctantly withdrew its conditions, since it had already entered into the experiment and agreed to participate in the initial experiment only.

According to the original planners, Mutual Agreement Programming (MAP) involves a crucial assessment of all inherent and salient personality manifestations of the inmate, stressing strengths and weaknesses, followed by a designed individualized program that will retard or augment said factors through utilizing existing and available therapeutic resources, thus preparing him for successful community reintegration following release on parole. Based on this assessment, treatment and training objectives are formulated and prescribed; the inmate prepares a plan for himself based on availability of resources, and contract negotiations involving this inmate, the institutional staff, the project coordinator, and the paroling authorities take place. The contract is, in effect, an agreement setting out the specific program that the institution will provide to the inmate, the inmate's agreement to successfully complete said program or other named objectives, and a negotiated parole date contingent on the successful completion of the specified goals.

This demonstrative effort conceptionalizes a selective, articulated parole procedure with parallel release criteria, designed to promote and increase agency cooperation and effectiveness and to improve the economic stability and positive community contributions of applicant offenders. MAP shifts obligatory responsibility to the inmate in meeting rehabilitative goals and envisions involving him from the very start of institutionalization.

On selection, the list of MAP eligibles transcended new admissions, those not yet heard by the Board, or already heard and denied parole, and those eligible for reinstatement on parole, having previously violated parole. The only criteria was that the inmate be within reasonable proximity of his parole eligibility date and that he/she did not have a detainer. Under the Arizona Revised Statutes 31-411, inmates serving an indeterminate sentence may, after serving one year, if this is equal to one-third of their minimum sentence, be released without time-credit deductions regardless of whether they are first offenders or repeaters. Inasmuch as MAP was introduced as an experimental program only, a control group was also selected for non-active participation geared at future comparisons.

In this experiment, the Board started scheduling active MAP applicants for contract negotiations beginning in December 1972, on receiving a list of selected eligibles. Each applicant appeared before the Board individually for contract negotiation. The MAP Project Coordinator, who guided development of individualized programs with each inmate, and a representative for the institution (Arizona State Prison or Safford Conservation Center) were also present. In each case where the Board, the institution and the inmate entered into a MAP Contract, a date of completion was set. Once the inmate had successfully complied with all the terms and conditions of his contract, he would again appear before the Board for finalization. At that time, the inmate would be formally granted parole by the Board with the parole conditions specified, subject to an approved program.

As of June 30, 1073, the Board had considered one hundred and eleven (111) MAP applicants. Eighty-two (82) contracts were signed, twenty-two (22) applicants were denied and seven (7) were passed. Fifty-nine (59) inmates completed their contract and three (3) MAP contracts were voided.

The Board's initial impression as to the concept of MAP was positive; believing that this project could well trend correctional thought. During participation, however, the Board became acutely aware of the many problems involved in developing an individualized program for each MAP applicant. Salient weaknesses in the program soon surfaced, and since final data appendicular to the control groups has not yet emerged and since experimental maturity has not yet been attained, an overall evaluation would prove ineffectual at this time. Studies relating to cause and effect and to available resources are still advisable.

APPENDIX V

SAMPLE INSTRUMENTS, ADMINISTRATION INSTRUCTIONS, AND DOCUMENT ACCOUNTING

Parole-Corrections Project   October, 1972

ADMINISTRATION OF QUESTIONAIRES AND FOLLOW-UP REPORTS

The three data collection instruments to be discussed in this paper are:

Intake Questionaire	Form # 01 (rev.)	(10/15/72)
Release Questionaire	Form # 41	(10/15/72)
Follow-Up Reports	Form # 51, 53, 56	(10/15/72)

I. Intake and Release Questionaires

A. General Instructions

The first two instruments are to be filled out by project participants with the help of an instructor. Hopefully, it will be possible for a single individual to administer all questionaires to participants in groups no larger than five. With an instructor entirely familiar with the questionaire, in a setting in which participants can ask questions and the instructor can closely monitor the activity, we hope to get a set of legible and intelligible forms.

The point should be made and repeatedly emphasized that all questions should be answered. If a subject is in doubt about some factual matter that can be checked from institution records, such as the date of his admission to prison, ask him to guess and place a question mark next to his answer, so that the project coordinator can later try to verify. If a subject is fully unable to guess (e.g., He has forgotten his Social Security Number.), have him enter a question mark in the blank rather than an answer, in order that it will be apparent the question was not simply overlooked.

Each subject should retain his own form until leaving the session, and subjects should be dismissed, one by one, by presenting their form to the instructor for inspection, so that he may have a last opportunity to check for completeness and legibility and assure that all data that can possibly be yielded are obtained before the subject has gone.

Following administration, the instructor and project coordinator should go over the forms. Omissions and obviously inconsistent or wrong factual information should be completed or corrected where possible by referring to the respondent's case file. Name and serial number should be checked for legibility. The respondent's project I.D. number should be entered on each page in the box in the upper right hand corner.

The date the questionaires were administered should be entered on the Status Report Form and the questionaires sent to the California Research Office in the monthly reporting package.

B. Specific Instructions for Intake Questionaire

The questionaire is set up with a related set of questions grouped together on one page. The instructor should explain each page to the participants and then wait until everyone has finished filling out the page before going on to the next one.

COVER PAGE — Identifying Information

PAGE 1 — Dates

With the exception of birth date, the format for all the other questions requires that one month and one year be circled on each line.

Q 1 -- Date of Birth

Q 2 -- Prison Admission Date: Date the man was admitted to prison for the current stay. If a man was admitted in 1968, released in 1970, and returned as a parole violator (i.e., without a new commitment offense) in 1971, the answer in Q 2 is 1971, not 1968.

Q 3 -- Minimum Eligible Parole Date: The earliest date the man could legally be released from prison. This is not necessarily the same as the legal minimum or the date of the first parole hearing. For some subjects, MEPD will be already past, and for other subjects still ahead.

Q 4 -- Estimated Release Date
Q 5 -- Estimated Release Date with MAP
The respondent is to estimate his probable release date (Q 4), and to make a second estimate supposing he were a MAP participant. (The two estimates may be the same.)

PAGE 2 — Descriptive Information

What is wanted in this set of questions is defined on the form, although some participants may require more explanation.

PAGE 3 — Employment, Past Job
PAGE 4a & b — Employment, Future Job
Training

Read the explanation at the top of each page to the participants. Emphasize the qualifiers in particular questions:

Q 14 & 20 "regular hourly wage (not overtime)"

Q 15 & 21 "average week"

Q 16 & 22 "take home pay" "average month"

Pay special attention to format in:

Q 17 — Requires two check marks, one for job experience, one for training.

Q 24 & 25 -- A "Yes" answer requires that the second part of the question be answered. The second part of Q 25 (describing training) is particularly important. Emphasize that answers should be as specific as possible.

PAGE 5 -- Importance of Release Date

Each of the questions on this page has a different format. Take time to explain the manner in which each is to be filled out.

PAGE 6 — Opinions about MAP

Emphasize that these are "opinion" not "information" questions.

C. Specific Instructions for Release Questionaire

The greatest difficulties with the Release Questionaire seem likely to be:

1. Getting advance notice of release date for controls and experimental dropouts so that they can be tested before leaving the institution. These procedures will have to be developed locally by the project coordinator at each institution.

2. Motivating subjects (especially controls) to sit down and answer the questions. Washington project headquarters will be asked to approve a token incentive plan at project expense so that project coordinators could provide, to every control subject or experimental subject who had not entered or had dropped out of contract status, a carton of cigarettes in exchange for a reasonably well-completed Release Questionaire. If approved, it will be necessary to determine some feasible way of actually making the payment.

Conditions for actual administration of the Release Questionaire, once subjects have been located and found willing, are likely to be less troublesome than for the Intake Questionaire, since the rate of flow of subjects out of the institution will be spread over a long period of time, and test administration can be conducted in individual sessions or with tiny groups. Further, the subjects will already have some familiarity and experience with the types of item being used, since many of the items are similar to those on the Intake Questionaire. For these reasons, it does not seem necessary at this time to develop detailed instructions for administering particular items. Project coordinators should familiarize themselves with the questionaire, and contact the research office for clarification if they are uncertain what information is desired or if difficulties arise during administration.

II. Follow-Up Reports

Three* copies of the Follow-Up Form should be prepared for each subject prior to his release from prison and are to be included in the case folder that ordinarily accompanies the offender from the institution to the parole agent, or forwarded separately to the agent if that is not feasible.

Pre-release preparation of forms is a simple task, requiring merely that several items of information on the cover page of each of the three forms be completed before the forms are forwarded to the field.

1. All three forms will carry the subject's name, prison serial number, project identification number, and prison release date.

2. On one of the three forms, circle "one" for "months since release" and write in "51" as the "Form No"; on the next form circle "three", and write in "53" as the "Form No."; and on the next, circle "six", and write in "56" as the "Form No.".

3. For each form, write in the date for "close of period". For example, if the subject's prison release date is January 14, 1973, write in February 14, 1973 as "close of period" for Form 51, April 14, 1973 on Form 53, and July 14, 1973 on Form 56.

4. Write in the "Due Date" on each form by checking the close of period date on each form, looking at a calendar, and setting the due date to fall on the first working day two weeks later. For the example given above, the Form 51 close of period date was Wednesday, February 14, so the Due Date is Wednesday, February 28. The Form 53 close of period date was Saturday, April 14, so the Due Date is Monday, April 30.

Procedures for distribution of forms for subjects on direct release or discharged without supervision are discussed in the Follow-Up section of Data System and Report Framework.

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* This requirement has been changed since the development of the October, 1972 "Data System and Report Framework", when four follow-up reports were required. Form # 52, calling for information on the subject's first two months on parole, has been dropped from the system.

ADDENDA (November 1, 1972)

Administration of Questionaires and Follow-up Reports

INTAKE QUESTIONAIRE

Questions 11 through 17 on page 3 are all in reference to a single job—the job mentioned in item 12 as the best the subject had in the year before coming to prison. Similarly, questions 18 through 26 on pages 4a and 4b are also all in reference to a single job--the one mentioned in item 18 as the job he'll try hardest to get soon after release.

RELEASE QUESTIONAIRE

Question 25 on page 5 has two parts. If the subject's answer to the first part is (0)--"no difference", the second part should not be answered. If he believes (1)-- that MAP reduced his stay in prison, or would have reduced it, he is to estimate the number of months saved in part two of the question; part two is answered in terms of months lost if he believes (2)-- that MAP increases time spent in prison.

Questions 1 through 9 on pages 1 and 2 are all in reference to a single job.

Section II on page 6 is to be completed only by subjects who obtained a MAP agreement, regardless of whether they stayed in MAP for the full period of the agreement. In responding to the question, each of the items 29 through 34 that the subject indicates was a term of his contract by checking "yes" under "part of contract" should also show a response for importance re: "getting a job" and a response for importance re: "staying out".

FOLLOW-UP FORMS

All three forms are to carry the project coordinator's name and mailing address.

All employment questions on the follow-up form are phrased in first-person because we assume this information will be provided by the subject in the agent's or caseworker's presence. The agent may either read the items to the subject and mark down his replies, or let the subject mark his own answers and assist him when he has difficulty.

ACA-DOL FORM 01 (REV.)
(10/15/72)

INTAKE QUESTIONAIRE
FOR PROJECT ELIGIBLES

____
Today's Date

Name____

Prison Serial Number____________________

Social Security Number ________________

Don't have one________ Don't remember________

ACA-DOL FORM 01 (REV.)
PAGE 1

/-------------------

1. Date of Birth________________________
month day year

2. In what month and year were you most recently admitted to or returned to prison? [CIRCLE ONE ON EACH LINE]

JAN FEB MAR APR MAY JUN JUL AUG SEPT OCT NOV DEC

1962 or before 1963 1964 1965 1966 1967 1968 1969 1970 1971 1972 1973

3. In what month and year is (or was) your minimum eligible parole date--(the earliest date you can legally be released from prison)? [CIRCLE ONE ON EACH LINE]

JAN FEB MAR APR MAY JUN JUL AUG SEPT OCT NOV DEC

1965 or before 1966 1967 1968 1969 1970 1971 1972 1973 1974 1975 1976 or after

4. Regardless of whether the parole board has given you a release date, make the best guess you can about the month and year you will be released. [CIRCLE ONE ON EACH LINE]

JAN FEB MAR APR MAY JUN JUL AUG SEPT OCT NOV DEC

1972 1973 1974 1975 1976 1977 1978 1979 1980 1981 1982 1983 or after

5. If you were interested in and accepted for a mutual agreement or contract program (MAP), what is your best guess about the month and year you would be released from prison? [CIRCLE ONE ON EACH LINE]

JAN FEB MAR APR MAY JUN JUL AUG SEPT OCT NOV DEC

1972 1973 1974 1975 1976 1977 1978 1979 1980 1981 1982 1983 or after

/----------

6. For what offense are you now under sentence? [IF MORE THAN ONE, CHECK THE ONE WHICH CARRIES THE LONGEST TERM.]

(1) _____ Murder
(2) _____ Robbery
(3) _____ Assault
(4) _____ Burglary
(5) _____ Theft (Exc. Auto)
(E) _____ Auto Theft
(N) _____ Forgery and Checks

(6) _____ Rape (Inc. Statutory)
(7) _____ Other Sex
(8) _____ Narcotics and Drugs (All types, inc. marijuana; Possession or Sale)
(0) _____ Technical Parole Offens
(9) _____ Other: _______________ Describe

7. Have you ever been paroled on the sentence for which you are now serving time?

(1) _____ Yes (2) _____ No

8. How many times have you been to prison? [DO NOT COUNT JAIL OR JUVENILE, BUT DO COUNT RETURNS TO PRISON FOR PAROLE VIOLATION.]

[CIRCLE ONE] 1 2 3 4 5 6 7 8 9 10

9. If you added up all of the time you have ever been locked up, how many years and months would it amount to? [INCLUDE TIME IN PRISON, JUVENILE FACILITIES, REFORMATORIES, DETENTION, AND JAIL.]

__________ Years __________ Months

10. Ethnic Group (Race) ________________________________

11. What's the highest grade in school you completed? [CIRCLE ONE]

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 More Than 16

/______________

THINK ABOUT THE BEST JOB YOU HAD IN THE YEAR BEFORE YOU CAME TO PRISON THIS TIME, AND ANSWER THESE QUESTIONS ABOUT THAT JOB. IN ANSWERING "TYPE OF JOB", USE A JOB TITLE OR DESCRIBE WHAT YOU DID. INCLUDE LEVEL OF SKILL IF POSSIBLE. FOR EXAMPLE: IF YOU WERE A CARPENTER'S HELPER, DON'T WRITE "CONSTRUCTION WORK" OR "CRAFTSMAN" OR "CARPENTER". WRITE "CARPENTER'S HELPER."

12. Type of job______________________________

13. How long were you on that job? [CHECK ONE]

(1) _____ less than 3 months
(2) _____ 3 to 6 months
(3) _____ 7 months to one year
(4) _____ more than one year

14. What was your regular hourly wage (not overtime)? [CHECK ONE]

(1) _____ less than $1.59
(2) _____ $1.60 to $1.99
(3) _____ $2.00 to $2.49
(4) _____ $2.50 to $2.99
(5) _____ $3.00 to $3.99
(6) _____ $4.00 to $4.99
(7) _____ $5.00 to $5.99
(8) _____ more than $6.00

15. How many hours did you work in an average week? [CHECK ONE]

(1) _____ less than 20 hrs.
(2) _____ 20 to 34 hrs.
(3) _____ 35 to 40 hrs.
(4) _____ 41 to 45 hrs.
(5) _____ 46 to 50 hrs.
(6) _____ over 50 hrs.

16. What was your take home pay for an average month? [CHECK ONE]

(1) _____ $200 or less
(2) _____ $201 to $300
(3) _____ $301 to $400
(4) _____ $401 to $500
(5) _____ $501 to $600
(6) _____ $601 to $700
(7) _____ $701 to $800
(8) _____ $801 or more

17. Since coming to prison this time, have you received any useful further job experience or training in this kind of work?

Prison Job Experience [CHECK ONE]	Training Other Than Job Exp. [CHECK ONE]
(1) _____ none received	(1) _____ none received
(2) _____ some received, but useless	(2) _____ some received, but useless
(3) _____ some received, and useful	(3) _____ some received, and useful

ACA-DOL FORM 01 (REV.)
PAGE 4a

THINK ABOUT THE KIND OF JOB YOU THINK YOU WILL TRY HARDEST TO GET SOON AFTER YOU ARE RELEASED FROM PRISON, AND ANSWER THESE QUESTIONS ABOUT THAT JOB. (GUESS IF YOU'RE NOT SURE.) AGAIN, USE A JOB TITLE OR DESCRIBE THE JOB AS WELL AS YOU CAN, INCLUDING LEVEL OF SKILL.

18. Type of job____

19. What do you think your chances are of getting this kind of job within six months after your release from prison? [CHECK ONE]

(1) _____ very good
(2) _____ good
(3) _____ poor
(4) _____ very poor

20. Estimate the regular hourly wage (not overtime) for this kind of job. [CHECK ONE]

(1) _____ less than $1.59
(2) _____ $1.60 to $1.99
(3) _____ $2.00 to $2.49
(4) _____ $2.50 to $2.99
(5) _____ $3.00 to $3.99
(6) _____ $4.00 to $4.99
(7) _____ $5.00 to $5.99
(8) _____ more than $6.00

21. Estimate how many hours you would work in an average week. [CHECK ONE]

(1) _____ less than 20 hrs.
(2) _____ 20 to 34 hrs.
(3) _____ 35 to 40 hrs.
(4) _____ 41 to 45 hrs.
(5) _____ 46 to 50 hrs.
(6) _____ over 50 hrs.

22. Estimate what your take home pay would be for an average month. [CHECK ONE]

(1) _____ $200 or less
(2) _____ $201 to $300
(3) _____ $301 to $400
(4) _____ $401 to $500
(5) _____ $501 to $600
(6) _____ $601 to $700
(7) _____ $701 to $800
(8) _____ $801 or more

23. Have you ever had this kind of job before coming to prison this time?

(1) _____ Yes (2) _____ No

/ ________________

24. Have you worked in or had on-the-job training for this kind of job during your present stay in prison?

(1) _____ Yes (2) _____ No

If "Yes", answer these questions

(1) How much were you paid per hour? $ __________

(2) How many hours a week did you work? __________ hrs. per week

(3) How many months were you on this job? __________ months

25. Have you taken courses during this stay in prison to prepare you for this kind of job?

(1) _____ Yes (2) _____ No

If "Yes", list them

NAME OF COURSE	MONTHS YOU SPENT IN COURSE
(1) ____________________	(1) ____________________
(2) ____________________	(2) ____________________
(3) ____________________	(3) ____________________

26. How much of your preparation for this kind of job was obtained during your present stay in prison? [CHECK ONE]

(1) _____ all of it
(2) _____ more than half
(3) _____ about half
(4) _____ less than half
(5) _____ very little
(6) _____ none of it

/--------

27. Suppose you were given ten dollars to bet on when you will be released, and were told you would win five dollars for every dollar you bet correctly. How many dollars would you bet on each of the following lines?

$______	$______	$______	$______	$______	$______	$______
Within 3 mos. from now	Between 4 & 6 from now	Between 7 & 9 from now	Between 10 & 12 from now	Between 13 & 15 from now	Between 16 & 18 from now	More tha 18 mos. from now

[MAKE SURE THE TOTAL BET ADDS UP TO $10.00]

28. How much trouble would you be willing to go to in order to know just when you will be released? [CHECK ONE]

(1) ______ none
(2) ______ very little
(3) ______ some
(4) ______ a fair amount
(5) ______ quite a lot
(6) ______ a hell of a lot

29. If you thought there was something you could do to reduce the amount of time you'll spend here before release, how much trouble would you be willing to go to in order to save:

		(1) NONE	(2) VERY LITTLE	(3) SOME	(4) A FAIR AMOUNT	(5) QUITE A LOT	(6 A HE OF A
[CHECK ONE]	1 mo.	______	______	______	______	______	______
[CHECK ONE]	3 mo.	______	______	______	______	______	______
[CHECK ONE]	6 mo.	______	______	______	______	______	______
[CHECK ONE]	9 mo.	______	______	______	______	______	______
[CHECK ONE]	12 mo.	______	______	______	______	______	______

[BE SURE YOU MADE 5 CHECK MARKS]

30. Compared to other prisoners whose offense and prior record are similar to your own, do you expect to serve: [CHECK ONE]

(1) ______ a lot less time before release
(2) ______ somewhat less time before release
(3) ______ about the same time before release
(4) ______ somewhat more time before release
(5) ______ a lot more time before release

THIS PAGE IS TO GET YOUR OPINIONS ABOUT MUTUAL AGREEMENT PROGRAMMING (MAP) OR CONTRACT PAROLE. CIRCLE T (TRUE) OR F (FALSE) NEXT TO EACH STATEMENT. IF YOU MARK THE LAST STATEMENT T, BE SURE TO CHECK ONE OF THE STATEMENTS FOLLOWING IT.

T F 31. The MAP program sounds like a bad idea to me.

T F 32. MAP sounds like a good idea, but I don't think it could help me.

T F 33. The parole board can be trusted to honor a MAP agreement.

T F 34. The institution can be trusted to honor a MAP agreement.

T F 35. Most prisoners can be trusted to honor a MAP agreement.

T F 36. Most prisoners at this institution have hardly any power to get the programs they want and feel they need.

T F 37. Most prisoners at this institution have a pretty good idea of when they'll be released.

T F 38. I'd like to get a MAP agreement.
→ [IF TRUE, CHECK ONE ANSWER BELOW.]

(1) _____ mainly to get out earlier
(2) _____ mainly to know my release date
(3) _____ mainly to get a better prison program

39. What are your present thoughts and feelings, questions, worries, etc., about the idea of contract programming?

____

____

____

____

____

____

____

____

(10/15/72)

PRISON RELEASE QUESTIONAIRE

FOR PROJECT EXPERIMENTALS AND CONTROLS

Section I: All Experimentals and Controls
Section II: Signed Contract Experimentals Only

____
Today's Date

Name ____

Prison Serial Number ______________

Social Security Number ______________

Don't Have one ________ Don't remember ________

Date you will be released from prison ____________________
month day year

Will you be _____ discharged or _____ paroled?

If paroled, name of parole agent ______________________

Parole Office ______________________________

WE WOULD LIKE TO BE ABLE TO CONTACT YOU AFTER YOUR RELEASE IN ORDER TO DO FOLLOW-UP RESEARCH ON THE MAP PROGRAM.

Residence after release ______________________________
Number Street

____
City State Telephone Number

IF YOU DON'T YET HAVE AN ADDRESS OR MIGHT MOVE FROM THE ONE ABOVE, IS THERE SOMEONE WE CAN CONTACT WHO WOULD KNOW HOW TO REACH YOU?

Name of person to contact ______________________________

____
Number Street City

____
State Telephone Number

ACA-DOL FORM 41
PAGE 1

/----------------

SECTION I: ALL EXPERIMENTALS AND CONTROLS

THINK ABOUT THE BEST JOB YOU THINK YOU CAN PROBABLY GET DURING YOUR FIRST SIX MONTHS ON PAROLE, AND ANSWER THESE QUESTIONS ABOUT THAT JOB. (GUESS IF YOU'RE NOT SURE.) IN ANSWERING "TYPE OF JOB" USE A JOB TITLE OR DESCRIBE WHAT YOU DID. INCLUDE LEVEL OF SKILL IF POSSIBLE. FOR EXAMPLE: IF YOU WERE A CARPENTER'S HELPER, DON'T WRITE "CONSTRUCTION WORK" OR "CRAFTSMAN" OR "CARPENTER". WRITE "CARPENTER'S HELPER".

1. Type of job ____

2. Estimate how long it will take you to get this kind of job. [CHECK ONE]

(0) _____ I've already got the job
(1) _____ less than one week
(2) _____ one or two weeks
(3) _____ three or four weeks
(4) _____ Between 1 and 2 months
(5) _____ Between 2 and 4 months
(6) _____ Between 4 and 6 months

IF YOU ALREADY HAVE THE JOB WAITING FOR YOU, WHICH OF THE FOLLOWING PEOPLE PLAYED AN IMPORTANT PART IN YOUR GETTING IT? [CHECK SEVERAL IF NECESSARY]

(1) _____ myself
(2) _____ a relative
(3) _____ a friend
(4) _____ a former employer
(5) _____ someone in the prison; WHO? ________________
(6) _____ my parole agent
(7) _____ Other; DESCRIBE: ________________

3. Estimate your hourly wage if you get this kind of job. [CHECK ONE]

(1) _____ less than $1.59
(2) _____ $1.60 to $1.99
(3) _____ $2.00 to $2.49
(4) _____ $2.50 to $2.99
(5) _____ $3.00 to $3.99
(6) _____ $4.00 to $4.99
(7) _____ $5.00 to $5.99
(8) _____ more than $6.00

4. How many hours a week would you expect to be working on this kind of job? [CHECK ONE]

(1) _____ less than 20 hrs.
(2) _____ 20 to 34 hrs.
(3) _____ 35 to 40 hrs.
(4) _____ 41 to 45 hrs.
(5) _____ 46 to 50 hrs.
(6) _____ over 50 hrs.

5. How much monthly take home pay do you expect from this job? [CHECK ONE]

(1) _____ $200 or less
(2) _____ $201 to $300
(3) _____ $301 to $400
(4) _____ $401 to $500
(5) _____ $501 to $600
(6) _____ $601 to $700
(7) _____ $701 to $800
(8) _____ $801 or more

6. Have you ever had this kind of job before coming to prison this time?

(1) _____ Yes (2) _____ No

7. Have you worked in or had on-the-job training for this kind of job during your present stay in prison?

(1) _____ Yes (2) _____ No

IF "YES", ANSWER THESE QUESTIONS:

(1) How much were you paid per hour? $__________

(2) How many hours a week did you work? __________hrs. per week

(3) How many months were you on this job? ________months

8. Have you taken courses during this stay in prison to prepare you for this kind of job?

(1) _____ Yes (2) _____ No

IF "YES", LIST THEM

NAME OF COURSE	MONTHS SPENT IN COURSE
(1) ____________________	(1) __________
(2) ____________________	(2) __________
(3) ____________________	(3) __________

9. How much of your preparation for this kind of job was obtained during your present stay in prison? [CHECK ONE]

(1) _____ all of it
(2) _____ more than half
(3) _____ about half
(4) _____ less than half
(5) _____ very little
(6) _____ none of it

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10. How much do you think low income or lack of work had to do with causing the offense that brought you to prison? [CHECK ONE]

(1) _____ none of the cause
(2) _____ a little
(3) _____ a fair amount
(4) _____ most of the cause

11. When you leave here, how much trouble do you expect to have with each of the following?

[CHECK ONE]	Finding a job you'll like	(1)___none	(2)___some	(3)___a lot
[CHECK ONE]	Finding a good place to live	(1)___none	(2)___some	(3)___a lot
[CHECK ONE]	Getting along with your family	(1)___none	(2)___some	(3)___a lot
[CHECK ONE]	Re-establishing old friendships	(1)___none	(2)___some	(3)___a lot
[CHECK ONE]	Avoiding criminal behavior	(1)___none	(2)___some	(3)___a lot

12. How much do you think the programs you've had in prison will help you to get a decent job after release and to stay out of prison in the future?

To Get a Decent Job [CHECK ONE]	To Stay Out of Prison [CHECK ONE]
(1) _____ no help at all	(1) _____ no help at all
(2) _____ a little	(2) _____ a little
(3) _____ a fair amount	(3) _____ a fair amount
(4) _____ a lot of help	(4) _____ a lot of help

13. Is there anything important that has happened to you during your present term in prison which you think may help you either to get a decent job after release or to stay out of prison in the future?

To Get a Decent Job	To Stay Out of Prison
(1) _____ Yes (2) _____ No	(1) _____ Yes (2) _____ No
IF "YES", DESCRIBE:	IF "YES", DESCRIBE:
______________________________	______________________________
______________________________	______________________________

14. In what specific occupations would you like to obtain training after you leave prison?

(0) _____ none

(1) Type of job____

(2) Type of job____

HOW MUCH DIFFERENCE DO YOU THINK GETTING AND HOLDING ONTO A MAP AGREEMENT MADE IN YOUR CASE? (IF YOU NEVER HAD A MAP AGREEMENT OR DROPPED OUT OF ONE, ANSWER THE QUESTIONS IN TERMS OF HOW MUCH DIFFERENCE YOU THINK GETTING AND HOLDING ONTO AN AGREEMENT WOULD HAVE MADE.)

Amount of Difference MAP Agreement Made or Would Have Made

[CHECK ONE ON EACH LINE]

	none	a little	a fair amount	a lot
15. Getting prison staff more interested in helping me	(1)___	(2)___	(3)___	(4)___
16. Getting myself more interested and working harder	(1)___	(2)___	(3)___	(4)___
17. Getting me into prison programs I couldn't have gotten without MAP	(1)___	(2)___	(3)___	(4)___
18. Making the programs I was already in work better	(1)___	(2)___	(3)___	(4)___
19. Making me more certain of my release date	(1)___	(2)___	(3)___	(4)___
20. Helping me plan & make arrangements outside because I knew when I'd be going home	(1)___	(2)___	(3)___	(4)___
21. Making my time in prison pass easier	(1)___	(2)___	(3)___	(4)___
22. Getting me out of prison earlier	(1)___	(2)___	(3)___	(4)___
23. Improving my chance of getting a good job after release	(1)___	(2)___	(3)___	(4)___
24. Helping me stay out of prison in the future	(1)___	(2)___	(3)___	(4)___

25. What effect did a MAP agreement have or would it have had on the length of your prison stay? [CHECK ONE]

(0) _____ no difference in prison time with MAP agreement
(1) _____ less prison time with MAP agreement
(2) _____ more prison time with MAP agreement

IF YOUR ANSWER WAS MORE TIME OR LESS TIME, HOW MANY MONTHS DIFFERENT? [CIRCLE ONE NUMBER]

1 2 3 4 5 6 7 8 9 10 11 12+

26. What things do you think are good about MAP?

27. What things do you think are bad about MAP?

28. Do you have any ideas on how MAP could be improved?

SECTION II: SIGNED CONTRACT EXPERIMENTALS ONLY

ANSWER THE NEXT QUESTIONS ONLY IF YOU HAD AN OFFICIAL SIGNED MAP AGREEMENT, BUT ANSWER THEM EVEN IF YOU LATER DROPPED THE AGREEMENT.

MAP AGREEMENTS WERE WRITTEN WITH TERMS IN ONE OR MORE OF THE FOLLOWING AREAS. MARK "YES" NEXT TO EACH OF THE TERMS THAT YOUR CONTRACT INCLUDED. FOR EACH "YES" YOU CHECK, RATE HOW IMPORTANT THAT PART OF THE CONTRACT WAS FOR GETTING A JOB AND FOR STAYING OUT OF PRISON IN THE FUTURE.

CHECK ONE; IF YES

AREA	PART OF CONTRACT (CHECK ONE) YES	NO	IMPORTANCE FOR: GETTING A JOB (CHECK ONE)	STAYING OUT (CHECK ONE)
29. Skill Training	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot
30. Education	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot
31. Job Assignment	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot
32. Treatment	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot
33. Discipline	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot
34. Other: ________ describe	☐	☐	(1)__none (3)__a fair amount (2)__little (4)__a lot	(1)__none (3)__a fa amou (2)__little (4)__a lot

Project Coordinator

ACA-DOL FORM
(10/15/72)

MUTUAL AGREEMENT PROGRAM (MAP)

Parole-Corrections Project

FOLLOW-UP FORM

Form No.______ Due Date______________

This report covers the period: ____________ TO ____________ ,
Prison Release   Close of Period

or the first ONE THREE SIX month(s) since release.
Circle one

For: Name______________________________Serial No.________

Filled out by: Name______________________________________

Agency____

NOTE:

This form is to be filled out by the subject's parole agent; or, if he is discharged, by his caseworker in another agency (if any), and if there is none, by himself.

Insofar as possible, the forms should follow the man:

1. If he is transferred in or out of state, to the new agent.
2. If he is discharged, to a caseworker or to the man himself.
3. If he absconds or is returned to prison, return with explanation to the Project Coordinator.

Three forms are to be completed for this subject, corresponding to the close of his first, third, and sixth month(s) in the community. Whenever the caseworker is in contact with the subject near the time a report is due, or expects such contact, he should remind himself to use the opportunity to collect the necessary information. For convenience, the caseworker should, as soon as the subject is released or transferred to his supervision, look at the three "Due Dates" on these forms, and note these dates on his calendar.

DIRECT ANY QUESTIONS TO THE PROJECT OFFICE LISTED ABOVE.

PAGE 1 /-----------

1. PLEASE CHECK ONE OF THE FOLLOWING WHICH APPLIES TO THE SUBJECT DURING THE PERIOD COVERED BY THE PARTICULAR REPORT. ADHERE TO THESE RULES:

If more than one thing has happened to the subject in the entire period since his release from prison, check the most serious. (No. [illegible] is the most serious, followed by Nos. 2,3,4,0, with - being the least serious.)

If a combination of things happen which are in different categories on the form, follow the same rule of checking the most serious. For example, S has been sentenced to serve 90 days in jail and 2 years on probation. Check 3, not 4.

If S is sentenced to a number of weekends in jail, the number of weekends times 2 days = jail time.

Remember, any amount of jail time which is suspended is checked 4; and only jail time which is served is to be counted in 3 and 4.

(1) _____ Returned to prison with a new commitment

(2) _____ Returned to prison on a technical violation

(3) _____ Parolee at Large (PAL) Absconder with felony warrant outstanding or felony charge unresolved
Declared criminally insane
Died in commission of a crime or from drug overdose
Convicted of an offense--
and sentenced to SERVE a jail term of 90 days or more;
or sentenced to 5 or more years on probation;
or received a SUSPENDED prison sentence

(4) _____ Arrested and/or held on parole charges only
Parolee at Large (PAL) Absconder--NO felony warrant outstanding or felony charge unresolved
Arrested and released
Convicted of an offense--
and sentenced to SERVE a jail term of under 90 days;
or sentenced to ANY AMOUNT of SUSPENDED jail time;
or sentenced to less than 5 years on probation;
or received a fine or forfeited bail

(0) _____ Arrested and charged with trial and/or sentence pending
Violated with parole board decision pending

(-) _____ No difficulty

2. What was the offense of which S was convicted, for which he was sent back on a technical violation or held in jail by the parole agent, or with which he is charged awaiting trial or capture?

____

3. If you are unable to fit S into any category above, describe his criminal envolvements during the current follow-up period. [USE BACK OF PAGE]

PAGE 2

THE FOLLOWING QUESTIONS ARE ABOUT EMPLOYMENT, AND SHOULD BE COMPLETED, IF AT ALL POSSIBLE, DURING AN INTERVIEW WITH THE SUBJECT. IF AN INTERVIEW IS, FOR SOME REASON, IMPOSSIBLE, THE CASEWORKER SHOULD ANSWER QUESTIONS ON THE BASIS OF THE MOST CURRENT INFORMATION AVAILABLE.

(1) _____ The following information is based on an interview with the subject on ____________________.
date

(2) _____ The following information is not based on direct interview with the subject. It was obtained (describe procedure):

____

____

____

4. What is your present work situation? [CHECK ONE]

(1) _____ employed full time
(2) _____ employed part time
(3) _____ unemployed
(4) _____ other; DESCRIBE ______________________________

5. How many different jobs have you had since you were released from prison? [CHECK ONE]

(1) _____ none
(2) _____ one
(3) _____ two
(4) _____ more than two

IF YOU HAVEN'T HAD A JOB SINCE RELEASE, IGNORE QUESTIONS # 6 THROUGH 12.

IF YOU ARE WORKING NOW, ANSWER THE FOLLOWING QUESTIONS ABOUT YOUR PRESENT JOB.

IF YOU HAD A JOB OR JOBS AFTER RELEASE, BUT AREN'T WORKING NOW, ANSWER QUESTIONS # 6 THROUGH 12 IN TERMS OF THE BEST JOB YOU HAD.

IN ANSWERING "TYPE OF JOB", USE A JOB TITLE OR DESCRIBE WHAT YOU DID. INCLUDE LEVEL OF SKILL IF POSSIBLE. FOR EXAMPLE, DON'T WRITE "CONSTRUCTION WORK" OR "CRAFTSMAN" OR "CARPENTER" IF YOU WERE A CARPENTER'S HELPER. WRITE "CARPENTER'S HELPER".

6. Job being described: [CHECK ONE]

(1) _____ present job

(2) _____ best job since release

IF (2), EXPLAIN WHY YOU NO LONGER HAVE THIS JOB:

____

7. Type of job ______________________________________

8. Regular hourly wage (not overtime): [CHECK ONE]

(1) _____ less than $1.59
(2) _____ $1.60 to $1.99
(3) _____ $2.00 to $2.49
(4) _____ $2.50 to $2.99
(5) _____ $3.00 to $3.99
(6) _____ $4.00 to $4.99
(7) _____ $5.00 to $5.99
(8) _____ more than $6.00

9. Average hours per week worked on this job: [CHECK ONE]

(1) _____ less than 20 hrs.
(2) _____ 20 to 34 hrs.
(3) _____ 35 to 40 hrs.
(4) _____ 41 to 45 hrs.
(5) _____ 46 to 50 hrs.
(6) _____ over 50 hrs.

10. Average TAKE-HOME pay PER MONTH on this job: [CHECK ONE]

(1) _____ $200 or less
(2) _____ $201 to $300
(3) _____ $301 to $400
(4) _____ $401 to $500
(5) _____ $501 to $600
(6) _____ $601 to $700
(7) _____ $701 to $800
(8) _____ $801 or more

11. How much of your preparation for this kind of job was obtained during your last stay in prison? [CHECK ONE]

(1) _____ all of it
(2) _____ more than half
(3) _____ about half
(4) _____ less than half
(5) _____ very little of it
(6) _____ none of it

12. How soon after release from prison did you take this job? [CHECK ONE]

(1) _____ less than one week
(2) _____ one or two weeks
(3) _____ three or four weeks
(4) _____ between 1 and 2 months
(5) _____ between 2 and 4 months
(6) _____ between 4 and 6 months

QUESTIONS ON THIS PAGE SHOULD BE ANSWERED BY EVERYONE WHETHER OR NOT EMPLOYED SINCE RELEASE.

13. Have you been enrolled in any education or job training courses or programs since your release from prison?

(1) _____ No (2) _____ Yes

IF "YES", LIST THEM BELOW:

Course or Program	Length of Course	Time you've been involved so far
1. __________	1. _____ mos.	1. _____ mos.
2. __________	2. _____ mos.	2. _____ mos.
3. __________	3. _____ mos.	3. _____ mos.

14. What kind of job will this training prepare you for?

Type of job ______________________________

15. What are your total earnings since the time of your release from prison? [CHECK ONE]

(1) _____ less than $100
(2) _____ $100 to $300
(3) _____ $301 to $500
(4) _____ $501 to $800
(5) _____ $ 801 to $1200
(6) _____ $1201 to $1700
(7) _____ $1701 to $2300
(8) _____ $2301 to $3000
(9) _____ $3001 to $4000
(10) _____ $4001 to $5000
(11) _____ $5001 to $6000
(12) _____ over $6000

16. Before your release from prison you were assigned to the Mutual Agreement Program (MAP) as an experimental or control subject. Has MAP played any part in making your job experience since release more pleasant or productive?

(1) _____ No. It didn't help at all.
(2) _____ Yes. It helped a small amount.
(3) _____ Yes. It helped quite a lot.

17. Did MAP help you in any way at all?

(1) _____ No (2) _____ Yes

IF "YES", HOW DID IT HELP?

____

____

Parole-Corrections Project

DATA SYSTEM AND REPORT FRAMEWORK

October, 1972

Project Stages

The project may be conceptualized as one which processes subjects through five sequential stages:

I. Intake Eligibility
II. Contract Preparation
III. Contract Implementation
IV. Release Processing
V. Community Follow-up

Research Reports

Each stage has a targeted date for closure (ie., the date the last subject should have completed the stage), and each stage is expected to be the topic of a research report at a later targeted date:

	Closure	Research Report
Stage I	month #6 (Feb, '73)	month #9 (3 mo. lag)
Stage II	month #7 (Mar, '73)	month #12 (5 mo. lag)
Stage III	month #12 (Aug, '73)	month #15 (3 mo. lag)
Stage IV	month #12 (Aug, '73)	month #21 (11 mo. lag)
Stage V	month #18 (Feb, '74)	month #24 (6 mo. lag)

Data Forms

Research reports will be based on data collection forms prepared during the respective project stages. At the end of each month, the project coordinators will assemble and forward to the research office all data forms for every subject who has completed any project stage in that month, and a status check on all subjects. Forms to be prepared and submitted for individual subjects include:

	Form	Form ID#
Stage I	Intake Questionaire	01 (Wisc); 01 revised (Ariz,Calif)
Stage II	Contract Term Sheets	21,22,23,24,25
Stage III	Contract Problem Resolution Form	31
Stage IV	Prison Release Questionaire	41
Stage V	Community Follow-up Sheet	51,52,53,56

The monthly status check will be accomplished on a form which permits cumulative recording for many subjects per page. The original record will be maintained by the project coordinator and remain in his possession. Each month the record (Form 10: Status Report Sheet) will be updated by the coordinator, with photocopies prepared and forwarded to the research office.

STATUS ORT SHEET

ACA-DOL FOR O
(10/16/72)

STATE ______________

STATUS ______________

______ of ______ PAGE

MONTH ______________

	1	2	3*	E 4					E 5*	E 6*	E 7												8*	9	10*			
				CONTRACT TERM PG.							PROGRESS REPORTS 0/1-6*														FOLLOW-UP			
#	NAME	ID#	IN.Q	1	2	3	4	5*	DROP	REN.	1	2	3	4	5	6	7	8	9	10	11	12	RE.Q	RE.D	1	2	3	4

Status Report Sheet

The project coordinator is to create and keep current a set of status report sheets for experimentals and controls on the enclosed set of forms.

State	Number of Subjects		Number of Sheets	
	Exp.	Con.	Exp.	Con.
Wisconsin	150	50	5	2
Arizona	144	72	5	3
California	200	50	7	2

Each month the master sheets are updated by the project coordinator. The sheets are photocopied, month filled in, and one copy of each set is sent to the Washington project office and one to the California research office. Reporting begins for the month in which the first Intake Questionaire was given and should be completed and forwarded by the first week of the month following. It is important that the activity is recorded on the sheet for the month in which it took place, since the form does not provide for exact dates in all columns.

Reporting Schedule

State	1st Report Due For Month of:	1st Report Due By:
Wisconsin	September	October 7*
Arizona	October	November 7
California	November	December 7

* The Wisconsin report will be completed in retrospect, and its submission necessarily delayed.

An explanation follows for filling out each column on the Status Report Sheet. An asterisk (*) on the Form signifies that additional documents are to be forwarded to the research office when entries are made in the column. An "E" signifies that the column applies to experimental subjects only.

1 After eligibles are randomized, enter the names of experimentals on one set of sheets and controls on the other. [See "Status" heading on Form.] Enter LAST NAME, followed by initial of first name. As intake proceeds beyond month 1, new names are simply added to the list on the sheets.

2 Enter project identification number

* 3 Intake Questionaire. Enter the date questionaire was taken. These are to be forwarded each month as completed after they have been checked and corrected by the project coordinator.

* E4 Five versions of the Contract Term Sheet (Form #s21-25) are required to achieve a completed contract:

Form #21 Prisoner Input
Form #22 Project Coordinator Input
Form #23 Institution Staff Input
Form #24 Semi-final Draft
Form #25 Final Terms of Agreement

As the successive versions are completed, they are filed and a check mark made in the appropriate column of the Status Report Sheet. When the final contract has been approved and signed by the parole board, prisoner, and institution representative, the date is entered in column 5 [i.e., check marks in columns E1-E4 of Status Report Sheet and date in column E5.] ALL FIVE VERSIONS (Forms 21-25) are sent to the research office. If the contract preparation process is aborted at an earlier stage and no contract achieved, the forms which were completed are forwarded, the corresponding columns checked, and the remaining columns left blank.

* E5 Drop. If the subject drops out of the program voluntarily, enter "V" and the date in column E5; if he is dropped non-voluntarily, enter "N" and the date. In either case, a separate document must be completed explaining the action, and that document forwarded to the research office. The explanation should be submitted on a Contract Problem Resolution Form (Form 31), with "Drop" shown in the Reason for Report section. Drops may occur either during contract preparation stages or after a contract has been secured and implementation begun.

* E6 Renegotiate. If at any point during the process, the contract is renegotiated, the date this action was initiated is entered in column E6. Two documents should be prepared and forwarded to the research office whenever a contract is formally renegotiated:

a. a new copy of Form 25 showing the renegotiated terms on the Contract Term Sheet.
b. a copy of Form 31 explaining the reasons for renegotiation on the Contract Problem Resolution Form.

Once renegotiation occurs, no more entries are to be made for the subject on the regular Status Report Sheet. Instead, his name is entered on the separate Renegotiated Contracts Status Report Sheet and subsequent entries of information for him made on that sheet. (If the same man renegotiates a second time, enter his name again and continue recording at the new location on the form.)

* E7 Each month for the first twelve months following contract signing, or until contract completion if it occurs in less than twelve months, an entry is made for each subject in a column of Section E7 of the Status Report Sheet. The entry for the subject's first month under contract will be made in column 1, the entry for his second month in column 2, etc.

If the project coordinator has received no written notice from an institution representative of less than satisfactory performance by the subject on any contract term during the particular month, a zero (0) will be entered for the subject in the column for that month. If less than satisfactory ratings were received, the number of separate contract term categories in which these occurred (Sill training, Work assignment, etc) will be counted, and the total (ranging from 1 to 6) will be entered. Whenever a less than satisfactory rating is received, a copy of the supporting documentation from the institution as well as a Contract Problem Resolution Form (Form 31) prepared by the project coordinator will be forwarded to the research office.

* 8 Release Questionaire. Enter the date the release questionaire was taken in column 8 RE.Q. Release questionaires are to be sent to the research office as soon as they have been checked over for completeness and corrected by the project coordinator.

[Note: Each project coordinator will be responsible for developing a reliable system to keep him aware of the approaching release of control subjects and experimental subjects who never entered or were later dropped from active contract status, in order that these may be scheduled for administration of the questionaire prior to release.]

9 Release Date. Enter the date the man was actually released from prison.

* 10 Follow-up Reports. A follow-up report (Forms 51-53,56) is due at the end of the subject's first, second, third, and sixth month following prison release. The procedures for collecting these data are:

I. For subjects on parole

1. The appropriate follow-up forms (a full set of four forms) will be inserted in the case folder to be kept in the possession of the parole agent. If the department of corrections operating system is such that the folder itself, or particular contents of the folder are routinely prepared in the institution and forwarded to the parole office, arrangements should be made to include the follow-up forms among the file contents being assembled, and prior to actual release from prison. If the project coordinator finds that some alternate arrangement is necessary, the new procedure should be checked with the research director.

2. The project coordinator will prepare four postcards, addressed to the parole office and agent responsible for case supervision of the subject, date these to correspond with the end of the subject's first, second, third, and six month after release, and insert these in a "tickler" file for automatic mailing to the parole agent, reminding him to prepare and submit follow-up reports when they are due. These reports

are to be mailed from the parole agent to the project coordinator.

3. If any follow-up report has not been received by the project coordinator by the time the reminder card for the subsequent report is taken from the tickler file for mailing, both the post card and a form letter noting the failure to submit the preceding report, and requesting compliance or explanation, will be mailed to the agent. If this action also proves unproductive by the time the next reminder card is to be mailed, the project coordinator is to alert the research office and take any other steps he feels appropriate to secure the required reports.

II. For subjects on direct release or discharged without supervision

1. If the subject is to be carried by another social agency (eg., DVR, ES, SW, MH) the follow-up report forms are forwarded to that agency and the above procedure followed. It will be necessary to determine the name and location of caseworker responsible for the subject.

2. If the subject will not be in regular contact with any social agency, he will be offered a set of four stamped envelopes addressed to the project coordinator, each envelope showing on its flap the month in which it is due, and containing a follow-up form. Subjects will be asked to take responsibility for preparing and returning the forms, and will also be asked for an address at which they can be reached or the address of someone likely to remain in touch with them so that reminder letters (not postcards open to public scrutiny) can be sent. Until we determine whether it will be possible to pay subjects for this participation, they should be told that it is possible that arrangements for payment can be made.

Status Reporting on Follow-up

The follow-up reports will be the most difficult part of project data collection so it is important that the project coordinator remain current on report delinquency.

1. If all four forms are received on schedule, a check mark is made in the appropriate column and a copy of each form forwarded to the research office as soon as received (i.e. with the monthly shipment of other forms completed) by the project coordinator.

2. If a form does not come in when due, and the form letter is sent, an "L" will be entered in the follow-up column identifying the missing report on the Status Report Sheet. "L" entries should be made in pencil so that they may be erased and replaced with check marks if, and when, the report eventually arrives.

Form # [check one]

__21 __22 __23 __24 __25

1. Skill Training

2. Work Assignment

3. Education

4. Treatment

5. Discipline

6. Other

Prisoner's Name ______________________________ Number ______________

Target Release Date _________________ Date this Sheet Prepared ____

Contract Term Sheets

These data sheets are identical to page 2 of the actual MAP contract, which provides headings under which to describe the specifics of terms undertaken in the areas of:

1. Skill Training
2. Work Assignment
3. Education
4. Treatment
5. Discipline
6. Other

The contract preparation process is seen as involving five stages, and a separate contract term data sheet is to be prepared at each stage. The basic data forms for all stages are identical, with the stage being identified by checking the appropriate form number in the upper right hand corner of the form.

First, the prisoner is invited to a brief session with the project coordinator for the purpose of considering the possibility of entering a MAP agreement. If interested, the subject is invited to develop his own terms on a form designated as #21. This task should be accomplished in the presence of the project coordinator, and while the coordinator may assist the subject, he is to resist any desire to influence the terms being set down. Form #21 is to represent as closely as possible the subject's own notions of a useful and desirable program.

Second, the project coordinator is to enter actively in discussion with the subject about the feasibility of the agreement being sought, and to make whatever suggestions about terms he believes are appropriate, but deferring to the subject's own judgment if in disagreement. As a result of this discussion, Form #22 is to be completed, representing a tentative program arrived at through collaboration and with input from the project coordinator.

Third, a copy of Form #22 is prepared for the purpose of checking feasibility of the tentative agreement with institution representatives who would be responsible for providing the service to permit satisfaction of specific contract terms. The originals of forms 21 and 22 will, meanwhile, have been stored in the subjec's project file. The results of the check on institution feasibility, including term modification, are to be entered on Form #23, which represents a program that the institution is prepared to provide, and indicates the parties responsible for providing it. [The prisoner and the project coordinator may divide the labor involved in determining the opinions of the parties necessary for particular terms. The process may take several days.]

Fourth, the subject and the project coordinator meet for a second session to discuss whatever modifications in the proposed agreement have occurred or been suggested on the basis of institution input, and whether the subject finds these modifications acceptable. At this time, Form #24 is prepared, with the project coordinator being careful to adhere as closely as possible to the subject's wishes, and to not over-ride his objections. Copies of Form #24 are made and submitted to the parole board and to the official institution represenative as the contract proposed for negotiation. The originals of forms 23 and 24 will at this time be stored in the subject's project file.

Fifth, the actual negotiation session will be scheduled, and an effort made to obtain a signed agreement. If the effort is successful, the actual contract term sheet will be copied and used as Form #25, and all five forms (#s 21,22,23,24,25) will then be forwarded to the research office. If negotiations are unsuccessful, a Form #25 will be prepared to indicate the terms which the parole board and institution sought to impose on the subject. If the process of contract preparation aborts at any of the earlier stages and is not resumed, copies of all term sheets prepared up to that point will be submitted to the research office.

[Note also procedures for recording information from these stages per: instructions for Status Report Sheet.]

ACA-DOL FORM 31

CONTRACT PROBLEM RESOLUTION FORM

________________________ Subject's Name

________________ Serial Number

1. Reason for report. [check one]

___1. "Less than satisfactory" rating received.
___2. Prisoner initiated a complaint.
___3. Other. [describe]

2. Problem occurred:

On contract term

___1. Skill Training
___2. Work Assignment
___3. Education
___4. Treatment
___5. Discipline
___6. Other

[check only one. submit separate form for each different term in which problem occurred.]

During

___1. 1st month of contr.
___2. 2nd " " "
___3. 3rd " " "
___4. 4th " " "
_____5th or later month [indicate which month]

[Briefly describe in the space below the nature of the problem as viewed by the prisoner, relevant institution representative, and project coordinator, including steps taken to resolve problem and nature of results obtained. Attach copy of prisoner's statement of complaint or institution representative's documentation for less than satisfactory rating on term. Attach copies of any other documents (eg., memoranda) that are directly pertinent.]

Contract Problem Resolution Form

Contracts will include terms from one or more of six categories (skill training, work assignment, education, treatment, discipline, and other.) Ordinarily, a single institution representative will be primarily responsible for the satisfaction of any given term (vocational instructor for skill training, a job supervisor for work assignment, etc.) and will monitor the prisoner's performance on that term. Each month, the appropriate institution representative is expected to make a judgment as to whether the subject is performing at a satisfactory or better level on the particular contract term, or at a less than satisfactory level. If the evaluation is "less than satisfactory", and appears to endanger the scheduled fulfillment of the contract, the institution representative is responsible for promptly notifying the project coordinator and providing a written statement of documentation of the problem. In parallel fashion, the prisoner himself may submit written complaints with regard to particular contract terms if he believes the institution is failing to deliver on programs or commitments to which it had agreed.

In the absence of information to the contrary, the project coordinator shall assume that contract performance is progressing satisfactorily for the month in question, and enter an "0" for the subject (zero terms in jeopardy) on the Status Report Sheet [Form #10.] Whenever a documented rating of less than satisfactory is received, or a complaint from the prisoner, the coordinator will conduct an inquiry, attempt to develop a solution acceptable to all parties, and prepare a written statement describing the steps taken and the outcome obtained on a Contract Problem Resolution Form [Form #31.] For any month in which a subject received one or more ratings of less than satisfactory, the number of contract terms in which such ratings were received [ie., from 1 to 6] will be entered for that subject on the Status Report Sheet, and a copy of both the separate rating documentations by the institution representatives for each term, together with a corresponding Contract Problem Resolution Form for each, will be forwarded to the research office. Written complaints prepared by the prisoner will also be copied and forwarded, together with accompanying CPR Form, but these will not result in any entry on the Status Report Sheet.

The project coordinator should generally alert all parties that problems must be promptly brought to his attention, and that any substantial delay in notifying him will be grounds for discounting or invalidating the seriousness of the problem.

SAMPLE FORM LETTER

[to be sent when a follow-up report is one month overdue.]

____
____
____

____
____
____

Dear __________________;

Follow-up Report ________ on Mr.[Ms.] ________________, #______________, due on ______________ is late. As you know, this subject participated in the MAP Contract Project. The follow-up data responsibility was assigned to you, and this information is crucial to our evaluation of the project.

We again request that you complete and submit the required form or notify us by phone or mail of the reasons why it is impossible to do so. Provisions exist on the form itself to indicate that the subject has been terminated from supervision or that his or her whereabouts are unknown. If the client has been transferred to another caseworker or agency, please forward this letter to the person now in charge of the case and also notify us of the name and address of the party now responsible.

Whatever the reason for difficulty in providing the follow-up, we would greatly appreciate your immediated reply and any assistance you can provide us so that we may find it possible to obtain the data from any available source.

Thank you for your help.

Sincerely

SAMPLE REMINDER POSTCARD

[Four cards are to be prepared and addressed at the time the subject is released, dated to correspond with the end of the 1st, 2nd, 3rd, and 6th month after release, and inserted in "tickler" file for automatic mailing to parole agent or other responsible caseworker.]

(date) __________

Dear ____________________,

Follow-up Report #________ on Mr.[Ms.] ______________________, who participated in the MAP program, is now due. The correct form should be found in the case folder. If you experience any difficulty in locating or filling out the report, please contact us immediately by telephone or letter

____
Project Coordinator

[Reverse side of card should have address of parole agent or caseworker and return address of project coordinator.]

DATA SYSTEM AND REPORT FORMAT ADDENDA JANUARY 20, 1973

Parole-Corrections Project California Research Office

Instructions for filling out Status Report Sheets

E4: Contract Term Page. The date wanted in Col. 5 is the Parole Board Hearing Date, i.e., the date the contract was signed.

E7: Progress Reports

1. For purposes of the progress reports, "month" refers to contract month for the individual man. Therefore, if the contract is signed by the Parole Board on Nov. 4, the reporting month ends Dec. 4, so a mark should appear in E7, Col. 1 on the December Status Report.

2. A recording must be made for each contract month. Only two types of information are recorded here: 1. that the man has received some number of "less than satisfactory" ratings in the areas on the Contract Term Page (record 1, 2, 3, 4, 5, or 6); or, 2. that he has not (record 0). Other types of problems calling for submission of a Contract Problem Resolution Form (Drops, inmate initiated complaints, difficulty getting into a program, etc.) are not recorded on this section of the Status Report Sheet.

ACA-DOL FORM (Rev. 1/73)

PART IV OBJECTIVES

CONTRACT TERM SHEET

Form # [Check One]
__21 __22 __23 __24 __25

NAME______________________ SERIAL #__________ ID #______

TARGET RELEASE DATE______________ DATE PREPARED______________

CONTRACT HEARING DATE [Form # 25 only]______________________

1. Skill Training

2. Work Assignment

3. Education

4. Treatment

5. Discipline

6. Other

ACA-DOL FORM 31 (Rev. 1/73)

CONTRACT PROBLEM RESOLUTION FORM

NAME_____________________________ SERIAL #__________ ID #______

DATE PROBLEM OCCURRED_________ DATE RESOLVED__________

REASON FOR REPORT [Check one]

_____1. "Less than satisfactory" rating [Note on Progress Report]
_____2. Prisoner initiated complaint
_____3. Other program component problem
[If 1, 2, or 3, check area in which problem occurred. Use separate sheet for each area.]
___a. Skill Training ___d. Treatment
___b. Work Assignment ___e. Discipline
___c. Education ___f. Other
_____4. Renegotiate
_____5. Drop--Voluntary
_____6. Drop--Non-voluntary
[If 5 or 6, check one below]
___a. Before contract obtained
___b. After contract obtained
_____7. Other type of problem. Describe:______________________

[Briefly describe in the space below the nature of the problem as viewed by the prisoner, relevant institution representative, and project coordinator, including steps taken to resolve problem and nature of results obtained. Attach copy of prisoner's statement of complaint or institution representative's documentation for less than satisfactory rating on term. Attach copies of any other documents (e.g., memoranda) that are directly pertinent.]

DATA SYSTEM AND REPORT FORMAT ADDENDA MARCH 14, 1973

Parole-Corrections Project California Research Office

Instructions for filling out Status Report Sheets

RECORDING CONVENTIONS

Use the following recording conventions for those cases in which required documents are missing.

Intake Questionnaire (3*) and Release Questionnaire (8*)

"Inmate refuses to fill out" - Enter "RUF" in box on Status Report
"Inmate released, escaped, or transferred before form filled out" - Enter "REL" in box on Status Report
"Other" - Enter "-" in box on Status Report

Contract Term Page (E4) and Follow-Up (10*)

When the Project Coordinator has determined that particular forms in these series will not be available, a "-" should be entered in the appropriate box on Status Report.

FOLLOW-UP FORM

The Follow-Up Form has been revised for use in Wisconsin and Arizona.

In all states, three and not four Follow-Up Forms are now required; therefore Col. 4 in 10* on the Status Report Sheet will always be blank.

CONTRACT COMPLETION

When a man with a contract is released, it will be assumed that the contract has been successfully completed unless a Contract Problem Resolution Form is submitted giving evidence to the contrary. The State Coordinators are to make this judgment.

RENEGOTIATED CONTRACTS

In the case in which a renegotiation if attempted, but rejected by either party and the original contract is retained, the Status Report is filled out in the following way.

1. Enter the man's name on the Renegotiation Form with a notation that the original contract has been retained.
2. Continue to do the progress recording for the man on the regular Status Report Sheet. There will be no progress recording on the Renegotiation Sheet in these cases.
3. DO NOT enter a date in the Renegotiate Box (E6*) on EITHER sheet.

4. Submit a Contract Problem Resolution Form explaining the circumstances.

If a renegotiation is attempted and fails, that is, the man is no longer under contract, this is entered on the Renegotiation Sheet as a Drop (N or V), and recording for the man continues on this sheet.

When an actual renegotiation has taken place, the date is entered on the regular Status Report Sheet and progress recording continued on the Renegotiation Sheet. DO NOT enter the date of this first renegotiation in Box 6 on the Renegotiation Sheet. (This is for the date of the second renegotiation should one occur.)

DATA SYSTEM AND REPORT FORMAT ADDENDUM # 3 MAY 3, 1973
Parole-Corrections Project California Research Office
Handling of Furlough Cases Arizona and California ONLY

1. Release from prison to furlough facility
    a. Submit Contract Problem Resolution Form.
    b. Record "F" and the date in Release Date box (Box 9) on the Status Report Sheet.

2. Release from furlough facility to parole or discharge
    a. Submit Contract Problem Resolution Form.
    b. Erase furlough release date in Box 9 and enter prison release date.
    c. Obtain a Release Questionnaire.
    d. Follow regular follow-up procedure.

3. Return to prison form furlough facility
    a. Submit Contract Problem Resolution Form explaining circumstances.
    b. Erase furlough release date in Box 9.
    c. No Release Questionnaire or Follow-Up Forms are required until the man is paroled or discharged.

DATA SYSTEM AND REPORT FORMAT ADDENDUM # 4 MAY 15, 1973

Parole-Corrections-Project California Research Office

Data Collection Instrument Revisions CALIFORNIA ONLY

Except for the Contract, the following existing versions of the instruments will be used. Modifications required in administration or use are noted.

INTAKE QUESTIONNAIRE 01 (REV 10/15/72)

Question 2: "Admission Date" means first trip from court to prison on this term.

Question 3: "MEPD" may be earlier than "Admission Date" because of jail and back time.

Questions 4 & 5: "Release Date" means release to parole or discharge, not furlough release date.

Question 11: GED will be written in if the man has one and has not completed the next higher grade.

Questions 24, 25, & 26: Prison Jobs or OJT

Question 34: Opinion about Institution Staff

These questions are not applicable in California since few of the men have had prison jobs or OJT (Ques. 24, 25, 26), and since there is no institution staff involvement in California MAP (Ques. 34). They will, therefore, be ignored in the analyses regardless of how they are answered. It is probably simplist for the questionnaire administrator to let the men answer the questions rather then risk confusion by telling them to ignore them.

RELEASE QUESTIONNAIRE 41 (10/15/73)

Questions 7, 8, & 9: These questions as written refer to prison job or OJT. Respondents will be instructed to fill them out describing jobs or OJT while in the community furlough facility.

FOLLOW-UP FORMS 51, 53, 56 (10/15/72)

No revision required.

STATUS REPORT SHEETS 10

No revision required except as noted on Handling of Furlough Cases Addendum and as noted below under Contract Term Pages.

CONTRACT AND CONTRACT TERM PAGES 21, 22, 24, 25 (REV 1/73)

CONTRACT: This has been revised and the new version accepted by all parties.

CONTRACT TERM PAGES: The format noted above will be used (copy attached). In California there will be 4 instead of 5 versions of the Contract Term Page. Form # 23: Institution Staff Input is not applicable. Box 3 in E4 on the Status Report Sheet will, therefore, always be blank.

DEFINITION OF CONTRACT TERM PAGES

Form # 21: The plan arrived at on the basis of initial discussion between coordinator and prisoner, and prior to confirmation about feasibility.
Form # 22: The plan as checked out in the field by the coordinator, found available, and presented to the prisoner for approval.
Form # 24: The proposed program presented to the Board for negotiation.
Form # 25: The final mutually approved terms of agreement.

CONTRACT PROBLEM RESOLUTION FORM 31 (REV 1/73)

This version of the form will be used (copy attached). The form does not include options for "Release to Furlough", "Release from Furlough", or "Return from Furlough"--each of which require submission of the CPRF (Addendum # 3: Handling of Furlough Cases). For these occurrances, "Other" should be checked and a full explanation of circumstances provided. All other instances in which a CPRF is required are noted in Data System and Report Format and various addenda.

MAP: RESEARCH DOCUMENTS--III. DATA COLLECTION INSTRUMENTS

A revised version of this sheet reflecting the changes described above is attached. Please replace the old sheet with the attached version.

ACA-DOL FORM (Rev. 1/73)

PART IV OBJECTIVES

CONTRACT TERM SHEET

Form # [Check One]

__21 __22 __23 __24 __25

NAME________________________________ SERIAL #__________ ID #______

TARGET RELEASE DATE______________DATE PREPARED___________________

CONTRACT HEARING DATE [Form # 25 only]__________________________

1. Skill Training

2. Work Assignment

3. Education

4. Treatment

5. Discipline

6. Other

ACA-DOL FORM 31 (Rev. 1/73)

CONTRACT PROBLEM RESOLUTION FORM

NAME________________________________ SERIAL #__________ ID #______

DATE PROBLEM OCCURRED_________ DATE RESOLVED_________

REASON FOR REPORT [Check one]

_____1. "Less than satisfactory" rating [Note on Progress Report]
_____2. Prisoner initiated complaint
_____3. Other program component problem
[If 1, 2, or 3, check area in which problem occurred. Use separate sheet for each area.]
___a. Skill Training ___d. Treatment
___b. Work Assignment ___e. Discipline
___c. Education ___f. Other
_____4. Renegotiate
_____5. Drop--Voluntary
_____6. Drop--Non-voluntary
[If 5 or 6, check one below]
___a. Before contract obtained
___b. After contract obtained
_____7. Other type of problem. Describe:________________________

[Briefly describe in the space below the nature of the problem as viewed by the prisoner, relevant institution representative, and project coordinator, including steps taken to resolve problem and nature of results obtained. Attach copy of prisoner's statement of complaint or institution representative's documentation for less than satisfactory rating on term. Attach copies of any other documents (e.g., memoranda) that are directly pertinent.]

NOTES ON MAP MOVEMENT SHEET — Monthly and Cumulative

1. Most of the information on this sheet is tabulated from the monthly Status Report Sheets submitted by each State. The row titles are outlined to show sub-totals.

2. "Active Contracts" and "In Progress" are cumulative tabulations obtained in the following ways:

Previous Month Active Contracts +
Current Month Contracts Achieved -
Current Month Completed (Released with completed contract) -
Current Month Drops after Contract =
Current Month Active Contracts

Previous Month In Progress +
Current Month Newly Randomized Experimentals -
Current Month Contracts Achieved -
Current Month Drops without Contract =
Current Month In Progress

3. Total Drops: Recombination of Drops after Contract and Drops without Contract.

4. Furlough Releases (Arizona and California only) Men counted here are always counted somewhere else on the sheet as well:
Returned: Carried as Active Contracts or Drop after Contract
Paroled/Discharged: Also entered in IV. Paroled/Discharged
Active: Carried as Active Contracts

5. Renegotiated Contracts: Tabulated in the month the initial contract was achieved. NOT re-tabulated as Contract Achieved for the Renegotiated Contract.

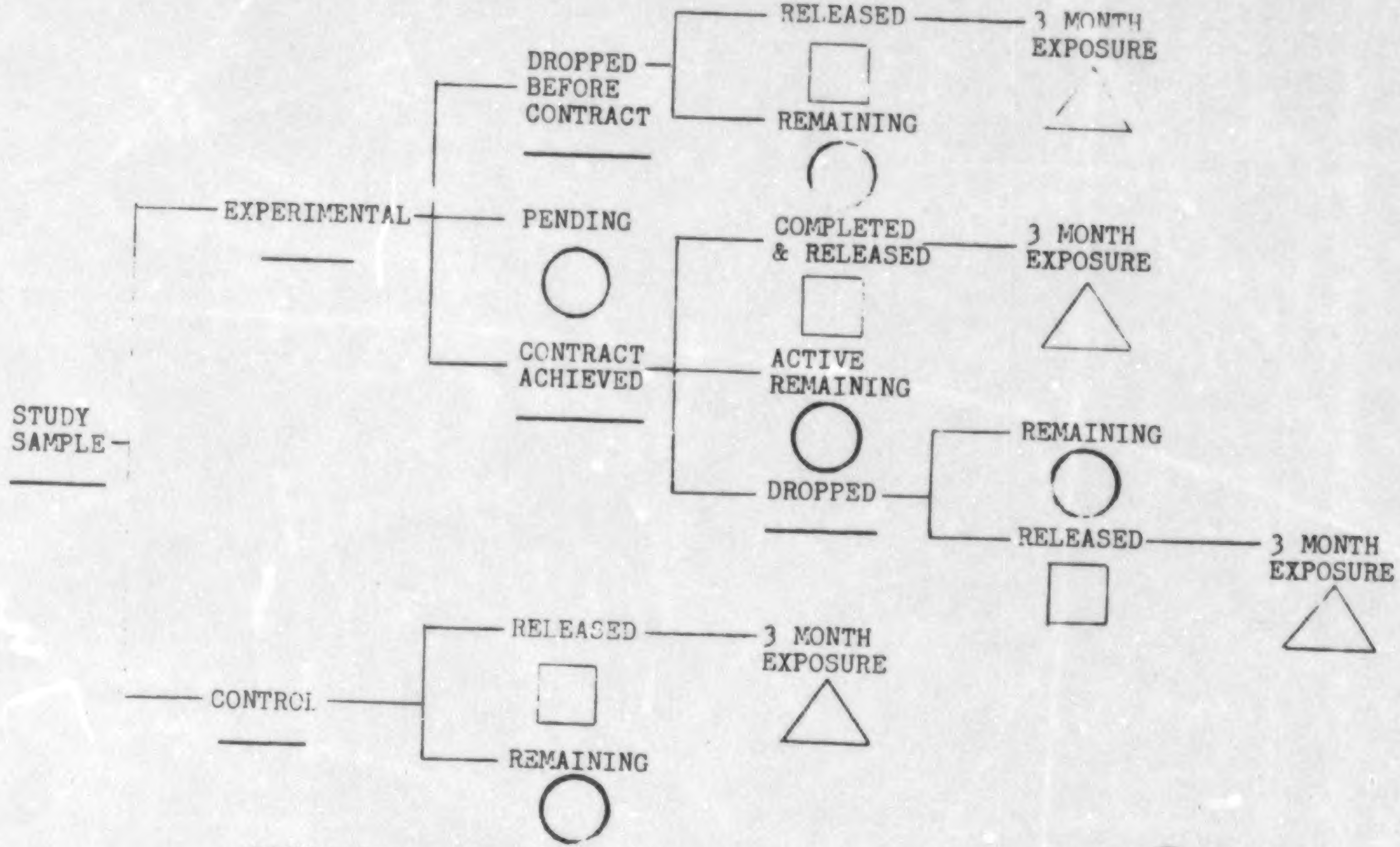
ACA-DOL 5/3/73

MAP: MOVEMENT SHEET

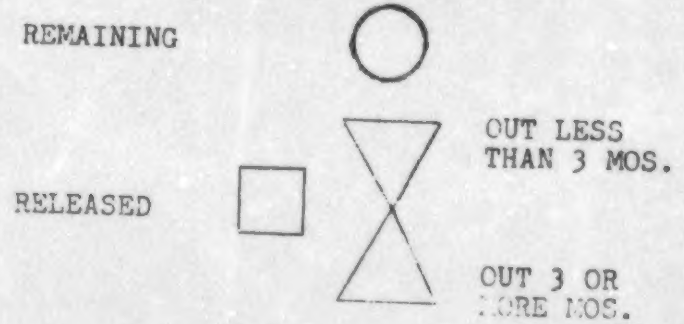
STATE______________________PERIOD______________

I. ELIGIBLE POOL				
A. Controls				
B. Experimentals				
1. Contracts Ach.				
a. Active				
b. Completed				
c. Drops				
Voluntary				
Non-Vol.				
2. Drops X Cont.				
a. Voluntary				
b. Non-Vol.				
3. In Progress				
II. TOTAL DROPS				
A. Voluntary				
B. Non-Voluntary				
III. FURLOUGH RELEASES				
A. Returned				
B. Par./Disc.				
C. Active				
IV. PAROLED/DISCHARGED				
A. Experimentals				
1. Completed				
2. Drops w/ Cont.				
3. Drops X Cont.				
B. Controls				

MAP FLOW CHART


STUDY SAMPLE
EXPERIMENTAL
DROPPED BEFORE CONTRACT
PENDING
CONTRACT ACHIEVED
RELEASED
REMAINING
3 MONTH EXPOSURE
COMPLETED & RELEASED
3 MONTH EXPOSURE
ACTIVE REMAINING
DROPPED
REMAINING
RELEASED
3 MONTH EXPOSURE
CONTROL
RELEASED
3 MONTH EXPOSURE
REMAINING

	DOCUMENTS EXPECTED	DOCUMENTS OBTAINED	O/E
INTAKE	____	____	____%
RELEASE	____	____	____%
FOLLOW-UP # 53	____	____	____%


REMAINING
RELEASED
OUT LESS THAN 3 MOS.
OUT 3 OR MORE MOS.

NOTES ON MAP DOCUMENT ACCOUNTING — Monthly and Cumulative

INTAKE This table shows the number of experimentals and controls randomized each month (# DUE) and the number of Intake Questionnaires obtained during each month (INT). The cumulative accounting shows the relationship between total questionniares due and questionnaires obtained to date for experimentals and controls.

RELEASE This table shows the number of experimentals and controls released each month (# DUE) and the number of Release Questionnaires obtained for that group of releases (REL). Questionnaires given before or after the actual release month will be recorded in the month the man is released. No questionnaire will be recorded until the man is released. Thus, the table shows the relationship between men released in a given month and questionnaires obtained for these men. The cumulative accounting shows the total number of experimentals and controls released and the total number of release questionnaires (obtained from men who have been released) to date.

FOLLOW-UP This table shows the month each Follow-Up form is due for the experimentals or controls released in a given month. The number of men released is recorded once in the # DUE column. The F-U column shows the number of questionnaires received for the particular release group. The month indicates only when the forms are due, not when they are received or recorded. The cumulative accounting shows the total number of Follow-Up Forms due and the total number of each type received to date.

ACA-DOL 5/3/73

MAP: DOCUMENT ACCOUNTING

STATE ____________________ PERIOD ____________

	# DUE	INT.	# DUE	INT.	# DUE	INT.	# DUE	INT.
TOTAL POPULATION								
Experimentals								
Controls								

	# DUE	REL.	# DUE	REL.	# DUE	REL.	# DUE	REL.
TOTAL RELEASES								
Experimentals								
Controls								

	# DUE	F-U	# DUE	F-U	# DUE	F-U	# DUE	F-U
FOLLOW-UP								
Form 51								
Form 53								
Form 56								